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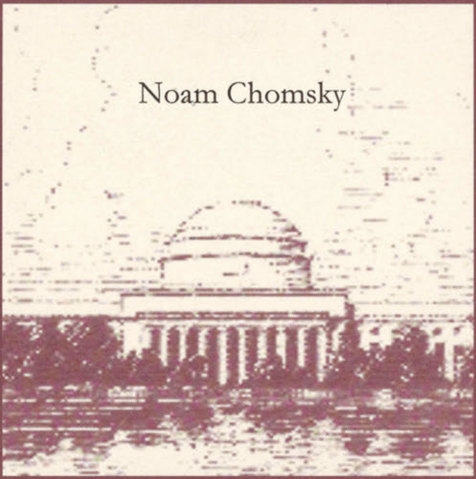

Noam Chomsky

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The Cybernetics Group

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Steve Joshua Heims

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Preface

The subject of this book is the series of multidisciplinary conferences, supported by the Macy Foundation and held between 1946 and 1953, to discuss a wide array of topics that eventually came to be called cybernetics. Coming in the aftermath of the Second World War, when the scientific and technical advances of the war years—for example, the modern general-purpose computer and models based on it—were just becoming public currency, the conference series played a significant historical role in the development of the human and the natural sciences in the United States.

The cybernetics conferences and attendant events form a complex story, and I have tried to include only a portion of it in this book. I have chosen to focus on researchers in psychology, anthropology, sociology, and psychiatry rather than on the engineers, biologists, and mathematicians.

For the book to be seen in its proper light, I need to say something about the process of writing it and my own relation to the subject matter. More than twenty years ago, as a physicist during the Vietnam War era, I felt a need to gain a broader perspective on the practice of the sciences and the direction they had taken in the postwar world. My method was twofold: to learn more about what people in other, related academic departments—anthropology, biology, psychology, mathematics—were up to and to acknowledge fully that science is a human activity, not only a body of knowledge. During this period the published proceedings of the cybernetics conferences fell into my hands, and since so many of the disciplines were represented by the attendees, a historical study of these meetings came to seem like a good way to focus my own inquiry.

I decided that it might be worthwhile to pursue my study in the form of a book, but I quickly saw that I was not yet ready to deal with the huge cast and variety of disciplines involved. I contented myself with writing a book about just two of the participants, the mathematicians John von Neumann and Norbert Wiener. When that book was finished, however, I felt encouraged to start work on the group of social scientists who had attended the meetings. Here I must add a warning: I have not practiced sociology, psychology, economics, psychiatry, or anthropology, and consequently I am looking at these fields as an outsider. My main interest is in what the people I discuss felt to be interesting and important at the time, not necessarily in what seems so today. Such an outsider's perspective can provide new insights, because it sidesteps the shared premises and practices within a discipline (recall Alexis de Tocqueville writing on America), but it also inevitably leads to a glossing over of many important details and technical points. To try to avoid major misunderstandings, I have consulted with specialists in the disciplines I discuss. This book, however, is not intended as a source of information about technical details. It is perhaps best characterized as the result of one person's historical examination and interpretation of portions of a very interesting conference series and of its participants.

One of my first steps was to contact as many of the participants as I could. Most, unfortunately, are now no longer alive. I began to read the participants' published writings, viewing them as contributions to "progress" within their specific disciplines. I also obtained whatever biographical information was readily available. But it didn't work. Much of the so-called social science was unconvincing to me as science in any traditional sense. In fact, some of it seemed to have only a thin scientific veneer, which apparently sufficed to make it acceptable. Moreover, as I wrote I found my study as a whole becoming centrifugal; it simply would not cohere. Something was wrong with my approach. Stymied, I put the manuscript aside.

When I returned to the project a few years later, I came at the subject matter in a different way, probably because I had picked up on changing attitudes among historians and sociologists of science. Instead of trying to review the specific contributions of individuals, I now started to look at fields as a whole and to explore the role of elite groups within fields, groups whose shared assumptions and consensus about what is valid and valuable establish the fields' priorities and guide the direction of research (including who gets funding, what gets published, etc.). From this point of view, conversations and discussions, including those at the center of this book and some at the periphery, took on a greater significance. I now saw that dialogue among researchers could serve as an organizing principle for my study. With this focus, the material I had gathered began to fall into discernible, seemingly natural, patterns. At various junctions in the book, where I had the data, I could now be specific, concrete, and explicit in describing instances of how the process of science worked.

Two kinds of presumed "background" to the conferences sometimes push themselves into the foreground as influences on the scientific work. One is the general political conditions in the United States at the time—the height of the Cold War—and more specifically, the general conditions of the various natural and social sciences. Chapter 1 describes these circumstances. The second is the intellectual interests each conferee brought to the first meeting. Chapter 2 is a systematic survey of those backgrounds. A reader who dislikes preliminaries might start with chapter 3. I expect, however, that sooner or later he or she will be impelled to turn back to the first two chapters for orientation.

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Acknowledgments

A large number of people have been helpful in the preparation of this book, which has been in process, intermittently, for many years. It is, in fact, the second half of the project begun in the book about von Neuman and Wiener, and some of the people are already mentioned there.

Harvey Shepard and Millard Clements, in their steady friendship, have been important through the years over and above any specific conversations. Leila Head, my daughter, with her professional devotion to the facts behind official obfuscation, is a continuing source of inspiration. Sharon Lamb, Susan Sklan, and Terri Payne Butler each read a chapter or more of the book critically, and each was a source of encouragement at a moment when it made a difference. The following people have contributed at one point or another, through conversation or by reading and commenting on a chapter or in some other way: Jonathan Bayliss, Iain Boal, Jean-Pierre Dupuy, Arthur Grant, Jay Haley, Joy Harvey, Rachel Joffe, Anita Landa, Mark Levine, John Lisman, Frank Manuel, Jennifer Markell, Robert Morison, Seymour Papert, Robbie Pfeufer, Edward Reed, Morris Schwartz, Silvan Schweber, Oliver Selfridge, Ann Sinclair, Michael Sokal, Lora Tessman, Erin Walsh, Sheldon White, Michael Wold, and William Woodward.

I greatly appreciate the interest shown by those members from the cybernetics group, most of them now deceased, who had taken time to talk or correspond with me. I am also grateful to other people who are not named individually here but have at some point during the past twenty-two years influenced or informed me in regard to some facet of the cybernetics group. I cite particular names and their contributions where appropriate in the notes. Archives at the Massachusetts Institute of Technology, Yale University, Harvard Medical School, the Library of Congress, the American Philosophical Society, and the National Library of Medicine were consulted in the course of the research, and I thank the archivists and librarians for their helpfulness.

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1

Midcentury, U.S.A.

The aim of this book is to describe a moment when a new set of ideas impinged on the human sciences and began to transform some traditional fields of inquiry. A proper description of this historical event entails not only attending to published research in the human sciences but also to individuals, to small groups, and to the larger social and political matrix in which the event was embedded. I will start the story with an overview of its particular setting: midcentury in the United States of America.

In the middle of the twentieth century the United States had reached a peak of power and standing among nations. Most of Europe, the Soviet Union, Japan, and China had been economically and physically ravaged by the Second World War, while the United States had emerged—as it had from the First World War—relatively unscathed and with a thriving economy. The war was generally felt by Americans to have been just or even righteous, and the country had made a special contribution to the victory by its technological know-how, managerial organization, and industrial production.

In the years following the end of the war in 1945, more than at any time since, Americans had faith in their government, were proud of their country, and felt secure in having a technological base that was second to none.[1] They were impressed by the country's newly acquired and unsurpassed status in the sciences. Scientific researchers, especially in applied physics, applied mathematics, and high-technology engineering, had returned from war work with high prestige. The population learned of their efforts from the mass media's treatment of scientists as quasi-heroes. Emerging high technologies included the electronic general-purpose computer and devices for more efficient communication.

With postwar prosperity the white middle class, with the nuclear family as its typical unit, began a move from the cities to the rapidly growing suburbs. Housing construction boomed as mass-production methods were introduced in the building industry. The efforts of the oil and automobile lobbies contributed to massive expansion of the highway system, as railroad transportation was replaced by automobiles, buses, and trucks. Big business thrived as total corporate assets multiplied. For a large proportion of the population it was a time of unprecedented affluence. The sociologist C. Wright Mills has described the period as "the Great American Celebration."

In the idiom of the day, applied social science was often referred to as "human engineering." In all, postwar circumstances were conducive to a ready acceptance of the political status quo and to a technological or technocratic optimism, even in the face of one frightening recent "success" in high technology—the atom bomb.

Meanwhile the global power of the European nations was sharply diminishing as they lost their colonies in Asia and Africa. The sole American colony, the Philippine Islands, was proclaimed an independent nation in 1945, even if other forms of hegemony, such as military bases, economic domination, and overt and covert political manipulation, continued. The United Nations, established in 1945, although wrapped in a great deal of internationalist sentiment and rhetoric, reflected in its structure control by the five leading powers of the postwar world (the United States, the Soviet Union, the United Kingdom, France, and China—the permanent members of the Security Council). It too provided an avenue of political influence of the leading nations over other countries, and among the leading nations the United States was second to none.

Within a few years, however, the internationalism—such as it was—gave way to the Cold War polarization that split the wartime alliance between the Soviet Union and the Western powers. American social and political thinkers reacted to this situation by adopting a split viewpoint—espousing internationalist ideology even as they took strong anti-Soviet stands. In this way internationalism, with its presumed promotion of political diversity, became confused and was sometimes transformed into advocacy for a Pax Americana. American social scientists were enlisted in the Cold War to help win the hearts and minds of people throughout the world away from Marxist political thought.

In the 1940s American anthropologists, sociologists, psychologists, and linguists had been mobilized for the war effort. The war "had opened up a whole new governmental activity as fields of inquiry for social scientists. Psychological warfare research, morale studies and propaganda analysis became firmly established as specialized fields."[2] For instance, "applied anthropology blossomed . . . anthropologists operated in their professional capacities in Military Intelligence, the Department of State, the Office of Strategic Services, the Board of Economic Warfare, the Strategic Bombing Survey, Military Government, the Selective Service Organization, the Office of Naval Intelligence, the Office of War Information, the Quartermaster Corps, the Federal Bureau of Investigation, the War Relocation Authority, the Alcan Highway Project, the Hydrographic Office of the Chief of Naval Operations, the Foreign Economic Administration, the Federal Security Administration, the medical branch of the Army Air Forces, and the Chemical Warfare Division."[3] The wartime mood was characterized by a "harmony of the ideological stances of the government with that dominant among the intellectual community." During the war the number of social scientists had grown considerably, and it continued to grow afterward. The membership of the American Psychological Association, for example, jumped from 2,600 to 4,000 during the war and increased dramatically thereafter; by 1960 it was well over 12,000, a fivefold increase in two decades. Clinical psychology in particular became an active, growing field, stimulated just after the war by money given to universities by the Veterans Administration and earmarked for the training of clinical psychologists, as well as by subsequent support from the U.S. Public Health Service and the National Science Foundation.[4] Seymour Sarason, a Yale University psychologist, has described the continuity from wartime to postwar conditions in the social sciences:

> For a short period after World War II the social sciences experienced remarkable growth in terms of numbers, funding, prestige and influence in the halls of public and private power. Social scientists were cocky and confident . . . sociologists enamored with grand, abstract theories of the structure and dynamics of society . . . psychologists promising much about their capacity to fathom the basic laws of de-

> velopment and behavior, to prevent individual abnormalities and miseries. . . . Anthropologists, who became instantly valuable to the government during World War II, became even more so with the war's end, when the nation emerged as the dominant military-social-political force in the world and took on administrative supervision of diverse peoples and cultures. Before World War II the social sciences were, except for economics, university based disciplines having, for all practical purposes, no ties with the political system. World War II changed all that; social scientists became needed and they wanted to be needed. They envied the status and support the natural sciences had gained by virtue of contributions to the war effort and the vistas these contributions opened for future "progress." The time had come, the social scientists argued, for our society to recognize that unless it took the lead to strengthen and support the social sciences we would miss a golden (and perhaps final) opportunity to gain that kind of knowledge and understanding necessary to reshape the social order, national and international, and to contain and even eliminate the destructive forces that had brought about World War II.[5]

It was a time when human sciences rather than humanistic studies were in the ascendancy, solving problems rather than reflecting on meanings. Normally the humanistic and scientific modes of understanding coexist, overlap, and are seen by the generalist as complementing each other. Yet the two modes also compete, and at certain times and places in Western intellectual history one mode has been strongly favored over the other. In the period following the Second World War in the United States, universities increasingly emphasized the social and behavioral sciences at the expense of humanistic scholarship. The phrase "behavioral science," invented in those days to replace "psychology," was indicative of psychology's tending to regard itself as a genuinely scientific discipline.

The flip side of America's conservative belief in itself was a growing hostility to social innovation. Increasingly the Soviet Union and all it represented was seen as a threat to the United States, especially in view of its controlling influence in Eastern Europe and the Soviet development of nuclear weapons. It was felt that the Soviet Union's expansionist tendency needed to be "contained." As Godfrey Hodgson put it,

> The frustration of having so much "power" and then finding that the world refused to be molded by it brought on the crisis we call after Senator Joseph McCarthy, though in fact it had begun long before he brandished his little list in Wheeling, West Virginia. The effects were as important inside the United States as they were for foreign policy.

> The Left was silenced. On the Right, orthodox conservatism was displaced by an obsession with unreal dangers. . . . The nation was committed to an ideology of anti-communism.[6]

Even American liberalism came in some respects to be shaped by anticommunism because, as historian Arthur Schlesinger observed, "the growing necessity of checking Communism by developing some constructive alternative speeded the clarification of liberal ideas in 1947 and 1948."[7] A sociologist revealed the pressure when he wrote in the 1950s, "as defenders of an alternate mode of life to that proposed by the Communists we are under additional compulsion to make our mode one which can integrate men of every color and culture."[8]

Whereas in Europe, Africa, Asia, and Latin America the intelligentsia was interested in studying and revising Marxist social science and dialectical thought generally (as had been true in the United States as well in the 1930s), many Americans now viewed Marxist ideas as dangerous. "Marxism and its practitioners were marginalized, if not completely banished from the academy."[9] A sharp change had occurred since the "popular front" coalition of leftists and liberals against European fascism in the latter half of the 1930s. Not only was Marxist thought unacceptable, but radical critiques and fundamental questioning of all kinds were muted within academic institutions.

At the time of the Macy conferences that are the focus of this book, some faculty members at U.S. universities, suspected of having once been communists, were being fired, while others were becoming anxious and cautious.[10] The academic establishment, concerned with the "good name" of its institutions, cooperated with the prevalent mood of anticommunism by requiring loyalty oaths or by creating committees to investigate the loyalties of suspect colleagues. Self-professed liberals at universities, to the disappointment of blacklisted professors, often did little to support academic freedom. The most substantial study to date of politics in the universities at that time concludes that

> The 1950s was the period when the nation's colleges and universities were becoming increasingly dependent upon and responsive toward the federal government. The academic community's collaboration with McCarthyism was part of that process. It was, in many respects, just another step in the integration of American higher education into the Cold War political system. . . . In its collaboration with Mc-

> Carthyism, the academic community behaved just like every other major institution in American life. . . . The academy's enforcement of McCarthyism had silenced an entire generation of radical intellectuals and snuffed out all meaningful opposition to the official version of the Cold War.[11]

While the political repression was felt heavily by employees of institutions such as universities, it did not prevent articulate criticism of the mainstream values and assumptions from outsiders. Despite or perhaps in reaction to the conservative mood of the mainstream, this was an innovative period in the arts. Out of the small counterculture of the early 1950s came Allen Ginsberg's beat poetry and Jack Kerouac's novels. Charlie Parker's musical innovations ("he engineered a total shift in the jazz aesthetic"[12]) at that time represented a summit in the evolution of jazz.[13] The late 1940s were also the high point in American abstract expressionist painting, when Jackson Pollock made his "poured" paintings in which an "unprecedented synthesis took place between Impressionism, Cubism and Surrealist automatism."[14]

Nor were dissident voices lacking from the nonacademic left. Sociologist and black leader W. E. B. DuBois, who had already devoted a long life to the black struggle, was a dissident voice exposing America's racism to the world; he was accordingly labeled a communist (which at that time he was not) and deprived of his passport, but never effectively silenced.[15] Paul Sweezy, a Marxist economist who had left Harvard to rely on independent means and edit the *Monthly Review,* sharply criticized capitalist economics and pointed to the embarrassing fact of poverty in America. The merits of these artists and intellectual dissidents were generally acknowledged only slowly and belatedly.

The close collaboration of natural and social scientists with the U.S. government during the war had made continued association seem natural at first, and the clear recognition of a divergence of interests between government and scholarly researchers was not always immediate. Paul Forman has made a convincing case that physics in the United States, from 1945 on, "accelerating its historical quantitative growth, underwent a qualitative change in its purposes and character, an enlistment and integration of the bulk of its practitioners and its practice in the nation's pursuit of security through ever more advanced military technologies."[16] Basic science was and continues to be lavishly supported by the weapons establishment because it had understood full well that "basic research, being but the applied research of tomorrow, is the key to technological progress."[17] In the period around 1950 the purposes of the government also impinged on the work of many social scientists, especially because the government had taken over from private foundations the funding of research. The spectrum of that relationship ranged from direct offers of work for the FBI and other intelligence agencies to being directly harassed and made unemployable by the anticommunist crusades. "In both the Soviet Union and the United States sociology was used as an instrument of state policy, both with respect to domestic problems and as an instrument for international leverage, influence and prestige. . . . The United States has done this increasingly since the growth of the Welfare State following World War II, and it has used the social sciences to check the spread of political and intellectual movements friendly toward Marxism and communism."[18] American anthropologists such as Margaret Mead helped to ferret out the weaknesses and vulnerabilities of the Soviet Union, but also advised on techniques for implementing foreign policies without creating unnecessary friction between the United States and other countries or cultures.[19] American cultural anthropology had at one time sought to overcome the colonialist tradition of the discipline; it had taught Americans about the wide range of variability in human nature and about the diversity of viable cultural patterns. But as the United States developed an active program of influencing countries in Africa, Latin America, and Asia within the framework of the "containment" of communism, the government once more sought the expertise of the anthropologists.

During the war, both natural and social scientists had typically worked as part of a team, often together with engineers, to address some particular interdisciplinary problem, such as the behavior, fatigue, and maneuvers of an aircraft pilot confronted with enemy planes in midair. Researchers came away from these wartime projects with some experience, perhaps even a habit, of interdisciplinary communication and collaboration, as well as a respect for many kinds of machines and their designers. In that regard the wartime experience would stand the conferees of the postwar cybernetics group in good stead.

We could characterize the epistemological framework and the cognitive style of the practice of science as reductionist, atomistic, positivistic, pragmatic, conservative, mechanistic, and empiricist. These attitudes manifested themselves in diverse ways in various branches of research. During the war, for example, physicists had quite ignored the outstanding prewar theoretical problem, the development of a satisfactory quantum field theory.[20] The needed experiments were carried out in 1946 and 1947 with great precision, using wartime advances in techniques for working with microwave radiation. The experiments preceded the major theoretical developments. A series of small conferences, by invitation only for an elite group of physicists, was set up under the sponsorship of the National Academy of Sciences, and by 1950 several gifted young physicists had devised theoretical techniques to compute—with a precision to match the experiments—subtle effects resulting from the interaction of electromagnetic fields with matter.[21] The theories, a major step forward in physics, were pragmatic in the sense that they provided rules for calculating empirically correct answers. Even though the mathematical and physical justification for the rules was problematic, the rules of calculation were readily accepted because they worked. American physicists' philosophy of science was mostly an implicit positivism, but epistemological questions—such as those raised by quantum theory—were shunted aside in the postwar years as dull compared to the doing of complex calculations.

Another group of physicists displayed their self-confidence after the war by invading the traditional domain of biology,[22] proposing to unravel the code-script embodied in the genes as suggested in Erwin Schrödinger's widely read book with the enticing title *What is Life?* (1944). A dedicated group of researchers who became known as the "phage group" formed around Max Delbrück and Salvador Luria and gathered in the summers at Cold Spring Harbor on Long Island; they were as narrowly goal-oriented as the designers of the atom bomb had been during the war. In this group young researchers obtained the training and orientation that eventually led to the discovery of the structure of the DNA molecule and creation of the new discipline of molecular biology. It was a highly reductionist, atomistic approach to problems of the nature of heredity and to the quest for understanding what life is. The optimism, even arrogance, of some of the participating physical scientists was a notable element in their success. Broader philosophical issues concerning the Darwinian theory of evolution and the variation of species moved temporarily into the background.

The tone of academic American sociology at midcentury was set by Talcott Parsons's functionalism, whereas critical Marxist sociology, insofar as it was practiced at all, survived only outside the academies.[23] Alvin Gouldner characterized Parsons's grand theories as "singularly insensitive to the sheer suffering of the desperately afflicted."[24] It was a conservative view centered on equilibrium, stability, and the continuity of institutions: "The usefulness of certain social or cultural arrangements for system equilibrium became focal."[25]

Cultural anthropologists had been conducting fieldwork among a wide variety of cultures, especially ones that seemed to face possible extinction—collecting them, recording them, classifying them nearly in the spirit of old-fashioned naturalists. The value of these studies was assumed to outweigh any harm that might come from intruding on otherwise isolated cultures. Both the older English anthropological functionalism and personality theory based on Freudian psychology were used to interpret cultures, but they were tempered with "relativism"—an insistence that each culture is its own framework within which its practices are to be understood. Yet the anthropologist using the empirical/theoretical/scientific/managerial framework of Western civilization to understand the relativity of cultures could hardly help assuming implicitly that the Western scientific world view was the true reality, the valid way of knowing, whereas those of other cultures were at best "interesting" or "scientific data," grist for the mill.

Atomism in the social sciences manifested itself in the tendency to reduce social and political issues to individual psychology, usually to Freudian psychoanalysis of the individual. Underlying the focus on individual behavior and psychology is the premise that the understanding of societies can be built up from the understanding of individuals, just as in physics the knowledge of atoms forms a basis for understanding macroscopic matter.[26] In the postwar years research interest shifted markedly from the social and political to the psychological, as presumably more fundamental.[27] The political issue of the oppression of minorities was turned into a problem of providing psychotherapy for people with "authoritarian personalities."[28] Shifting what might have been controversial political issues to apparently "scientific" facts of psychology seemed to provide a form of reductionism that made social issues manageable. Atomism and reduction to individual psychology were allied to the "mental health movement" that was so optimistic after the end of the war:

> An effective safeguard against war would be an international research institute devoted to the study of individual and group aggression, hatred and fear and their etiology in terms of personality development, cultural heritage, and social conditions.[29]

Unlike clinical psychology, where Freud's ideas reigned, academic psychology was dominated in the postwar years by neobehaviorism, modifications of the earlier strict behaviorism of John Watson.[30] The classic experiments of Pavlov in Russia had provided the foundation for behaviorist theories. Among the leading neobehaviorists Edward Chace Tolman was exceptional in his independent, liberal revision of behaviorism, attributing central importance to the concepts of "purpose" and "cognition" in describing an animal's behavior, and in his broad appreciation of other schools of psychology, including gestalt psychology and psychoanalysis. Watson's behaviorism had been thoroughly objectivist, reductionist, and empiricist. His objective, a psychology that would lead to prediction and control of behavior, was congenial to practical men who would manage people. According to behaviorist tenets, psychology was to resemble physics in its methodology, mental events are not objectively observed and have no place in scientific description, while behavior—the moving of particular muscles—is to be understood as the result of particular stimuli (past and present). Behaviorism focuses on the relation of an organism's actions to the environment and pays scant attention to internal states. Behaviorism in psychology became allied with a logical positivist philosophy of science. Their similar historical trajectories have been summarized by Laurence Smith:

> By the late thirties, both were clearly the dominant orientation within their disciplines, and their periods of ascendancy continued through the 1940s. In the mid-thirties, both movements began to undergo liberalizations of their formerly more strict formulations. By the fifties, there was a growing recognition of how seriously these liberalizations compromised the original founding principles of each movement. Coupled with continuing criticism from outside, this recognition contributed to a decline of influence through the fifties.[31]

At midcentury it was the ideas from cybernetics, computer models of cognitive processes, and information theory that successfully challenged the prevailing academic psychology.

We have noted how the postwar circumstance of social scientists was such as to enhance their belief in their own wisdom, aside from their participation in the general technocratic optimism of the period. Having left interdisciplinary wartime projects, they had returned to academic departments representing only their own special disciplines. They were nevertheless eager to shore up their always somewhat shaky identity as "scientists," to assimilate to the more prestigious physical and biological sciences, and they were willing to learn from mathematics, the natural sciences, and engineering how to make their own practice more scientific. The objectives of the quantum field theorists as well as those of the phage group were specific and well-defined, the discussion narrowly focused, and the work highly technical. These groups offered no link to or ingress for social and behavioral scientists. But the work of some mathematicians, engineers, and physiologists did provide such a connection.

A group of men who had, during the war years, formed a network based on common scientific interests included several mathematicians (Norbert Wiener, John von Neumann), engineers (Julian Bigelow, Claude Shannon), neurobiologists (Rafael Lorente de Nó, Arturo Rosenblueth), a neuropsychiatrist (Warren McCulloch), and a polymathic genius (Walter Pitts). Some members of this group had proposed that their concepts, useful in engineering and biology, had more general significance, perhaps even could provide tools for a transdisciplinary synthesis that might be of particular interest to researchers in the human sciences. For lack of a better collective name we shall refer to this group as the cyberneticians, although they would never have used this term themselves (each one was still identified with a traditional discipline) and the word *cybernetics* for the new science was not introduced until 1947. What is of interest, however, is that they constituted a kind of community with a shared idiom, that they were talking to each other over a period of years, and that they had reached a consensus on the scientific importance of a set of notions, which will be described in this book. Although John von Neumann and Norbert Wiener were central figures in the group, they are the subject of a previous volume and will be given relatively short shrift here.[32]

All of this group was interested in models of the brain based on electronic circuitry and inclined to mechanistic philosophy. Discussions between Wiener and von Neumann about the relative merits of working on the theory of bacteriophage as opposed to neurobiology, as well as the theories they each put forth, indicate that von Neumann was more reductionist in outlook than Wiener.[33] Unlike Wiener, who viewed science as a means for describing natural process, von Neumann was also in the logical positivist tradition. Wiener's approach to psychology, not unlike Tolman's, was close to behavioristic, even though he insisted on using the imprecisely defined concept of a "purpose" that an organism might have in engaging in particular actions. He liked to formulate psychology in terms of cybernetic concepts, but he was open to the evidence of gestalt psychology as well as to Freudian psychoanalysis. It is this group of cyberneticians, Wiener and von Neumann among them, who met regularly between 1946 and 1953 with, among others, representatives from the human sciences at the series of conferences that are the subject of this book.

The cyberneticians formed one cluster of people at the Macy conferences on cybernetics. One other group formed a cluster that guided the selection of representatives of the human sciences for the conferences. Lawrence K. Frank, Margaret Mead, and Gregory Bateson, at the core of that cluster, had a substantial consensus on what is worthwhile in the human sciences. They were not devotees of behaviorism, but part of the "personality and culture" movement. They agreed that some of the concepts of the cyberneticians were likely to be valuable in developing conceptual schemes in the human sciences and would help to make their disciplines more rigorous.

It is my standpoint in the history of science, and especially of the human sciences, that such clusters among elite groups talking with each other, influencing each other, forming some mutual consensus about what is important, how to look at things, in what direction progress is needed, and using their collective resources and prestige to bring some area of research forward are a significant part of the process. Studying the interactions of such clusters—as the Macy meetings allow us to do—offers a good counterweight to the sort of history one would write solely on the basis of published research results. Indeed, it is my view that such result-oriented history misrepresents the nature of these sciences.

It is a corollary of the positivist approach to understanding science that certain "unpopular" investigations and lines of research must be ignored. Here is one reason to take a contextual approach in the history of science, describing what was done and thought at the time in its own terms, rather than picking out only what is considered important by today's scientists. What was valued a few decades ago but not today may turn out to be important next year. This point will become clear as we examine the history of the discussions at the Macy meetings.

2

March 8–9, 1946

The cyberneticians and the Frank-Mead-Bateson cluster of social scientists first encountered each other in New York City in May 1942.[1] The occasion had been a small interdisciplinary meeting, by invitation only, on two topics: the always-suspect subject of hypnosis and the respectable topic of the physiology of the conditioned reflex (the basic unit of behaviorist psychology).[2] Howard Liddell, an experimenter on the conditioning and behavior of mammals, and Milton Erickson, an innovator in the use of hypnotism, lead the discussion.[3]

The Cerebral Inhibition Meeting, as it was officially called, had been arranged by the medical director of the Josiah Macy Jr. Foundation, Frank Fremont-Smith. He had been for some years in professional dialogue with neurophysiologists among the cyberneticians, and (especially through L. K. Frank, who had been his associate and mentor at the Macy Foundation) with representatives of the social science cluster. So it came about, with Fremont-Smith as liaison, that at the 1941 meeting the two intellectual universes encountered and began to penetrate each other. From the cyberneticians Warren McCulloch and Arturo Rosenblueth were present, and from the core group of social scientists Gregory Bateson, Margaret Mead, and Lawrence K. Frank attended. Also present was Lawrence Kubie, well known to Fremont-Smith for many years. He was not at the core of either group, but, partly because of a change of profession in mid-life from neurophysiologist to psychoanalyst, was acquainted and conversant with portions of the idiom of both groups.

Historical interest in this little meeting derives from Rosenblueth's presentation, which was the seed that instigated the postwar series of conferences on cybernetics and from which the interdisciplinary field, cybernetics, evolved.[4] Mead later recalled the excitement it produced among the social scientists, with the comment, “I did not notice that I had broken one of my teeth until the Conference was over.”[5]

Rosenblueth presented to the meeting the upshot of his conversations with Norbert Wiener and Julian Bigelow, namely a general concept, or more precisely, a model, that encompassed certain engineering devices as well as aspects of human behavior.[6] The concept seemed sufficiently cogent that, if it were taken seriously, a major program for interdisciplinary research might well spring from it. Some of the group of listeners at the Cerebral Inhibition Meeting responded instantly to Rosenblueth’s enthusiasm and were persuaded of the possibilities of the paradigm for interdisciplinary research.

Essentially the idea was to identify in a behaviorist spirit some of those aspects of what organisms do that can be analyzed in terms of what certain analogous machines do.[7] But the analysis differed in some important respects from the tenets of classical behaviorism. First, it was concerned with goal-directed actions, where an organism acts with a “purpose,” although, as Rosenblueth and collaborators put it, “the definition of purposeful behavior is relatively vague, and hence operationally largely meaningless, the concept of purpose is useful and should, therefore, be retained.” Explaining actions in terms of a goal to be attained had traditionally been criticized by scientists because it meant explaining actions in terms of events that had not yet happened, the cause, so to speak, coming after the effect. Rosenblueth and his friends rejected the criticisms as irrelevant and readily spoke of goal-directed actions as in a well-defined sense “teleological.” The description of purposive behavior of organisms in the images and language of engineering meant that, notwithstanding the traditional opposition between teleology and mechanism, one could henceforth speak explicitly and concretely about “teleological mechanisms.”

Second, the model replaced the traditional cause-and-effect relation of a stimulus leading to a response by a “circular causality” requiring negative feedback: A person reaches for a glass of water to pick it up, and as she extends her arm and hand is continuously informed (negative feedback)—by visual or proprioceptive sensations—how close the hand is to the glass and then guides the action accordingly, so as to achieve the goal of smoothly grabbing the glass. The process is circular because the position of the arm and hand achieved at one moment is part of the input information for the action at the next moment. If the circuit is intact, it regulates the process. To give another stock example, when a man is steering a ship, the person, the compass, the ship's engine, and the rudder are all part of the goal-directed system with feedback. The machine is part of the circuit. A still different case, an automatic pilot, does not involve a person anywhere in the circuit—it is all machine. Rosenblueth in his talk singled out goal-directed circular-causal processes with negative feedback as commonplace and worthy of systematic investigation in both organisms and machines, as well as in combined machine-organism systems.

In the physical sciences problems are traditionally formulated in terms of cause and effect: A causes B, forces cause acceleration. These formulations had led to descriptions in terms of a tractable (linear) mathematics, beginning with the theories of Isaac Newton in the seventeenth century. Circular causality was avoided because it seemed prohibitively difficult mathematically, nor did it seem necessary—although it had already appeared in the General Theory of Relativity. Many scientists came to act as if they believed that the world accommodated to what their mathematics could handle and ignored the rest. Of course it does not. By pointing out that a large class of ordinary phenomena exhibit circular causality, but can be described mathematically, and that one need not be intimidated by the mathematics, the Rosenblueth-Wiener-Bigelow ideas seemed to have vastly extended the realm accessible to exact science.

Rosenblueth and Warren McCulloch were close in their professional interests, and had in the previous year planned to do some experiments together.[8] Rosenblueth's presentation brought them even closer, as they were moving in similar directions. McCulloch was enthusiastic about Rosenblueth's talk, especially as the modelling of human behavior (necessarily involving elements of the nervous system) was put into the same category as some engineering devices. McCulloch had himself been thinking about hypothetical engineering devices (a class of general-purpose computers) to model the human mind and brain. While the origins of a person's purposes, presumably located within mind and brain, was a topic outside of Rosenblueth's model, they might be describable in terms of the model McCulloch was working on at the time. Of course, such a model concerned with mind would not fit a behaviorist framework, even though it might connect directly with the ideas Rosenblueth had presented, in which mind and brain were in effect a black box.

The Rosenblueth-Wiener-Bigelow model had also intrigued conferee Gregory Bateson. He thought that he could see in it the seed of an approach to anthropology, sociology, learning, and language that could generate much-needed theory in these fields. The dynamic of one arc of a feedback loop referring to a person, another portion to a signal carrying information, and the rest to the environment, with all included in a model so clear it could be formulated in mathematical language, appealed to Bateson as a potentially fruitful mode of description. Mead, L. K. Frank, Fremont-Smith, and Kubie were also interested in pursuing the concepts.

At the time of the Cerebral Inhibition Meeting, May 1942, Wiener and Bigelow were already working on military projects, and some of the social scientists were about to become involved in war work. Interdisciplinary discussion of the ideas was delayed until after the war. Shortly after Japan had surrendered, Fremont-Smith, at the urging of McCulloch, began planning a conference to explore the ideas Rosenblueth had presented four years earlier.[9] Presumably, McCulloch's own ideas would also be aired. The conference plans might have been limited to the subjects of laboratory physiology, neuropsychiatry, and mathematics had Bateson not returned from duties in Southeast Asia in time to prevail on Fremont-Smith to include social and behavioral scientists among the small group of invitees. The Josiah Macy Jr. Foundation, with Fremont-Smith its medical director and chief conference organizer, would make all arrangements and reimburse participants the cost of travel and hotel. The Feedback Mechanisms and Circular Causal Systems in Biology and the Social Sciences Meeting, which took place 8–9 March 1946, promised to generate a new kind of link between engineering, biology, and mathematics on the one hand and psychology, psychiatry, and all the social sciences on the other. It turned out to be a major intellectual event and was followed up over a period of seven years by nine additional meetings of the same group of people. These subsequent meetings, although important, were anticlimactic after the first.

All the conferences were held in the Beekman Hotel at 575 Park Avenue, New York City, except for the last, which took place at the Nassau Inn in Princeton, New Jersey, to accommodate John von Neumann. Fremont-Smith arranged with the

Macy Foundation, which had offices near the Beekman, to sponsor the meetings and provide transportation expenses, hotel rooms, and meals and cocktails for all participants. He established the format of the conferences: approximately twenty regular participants to be supplemented at each meeting by a few guests. Meetings were for two full days. The last five meetings were recorded by a stenotypist and published after some editing by conferees von Förster, Mead, and Teuber. Technically unsuccessful attempts had been made to record the first few conferences mechanically and transcribe them. Margaret Mead took detailed notes, but they are in her own shorthand. Consequently the most useful sources of information about the first five conferences are the available summaries and the considerable correspondence generated by the meetings.

The elaboration, critique, extension, refinement, and following out the implications of the material presented at that March 1946 meeting began at this series of conferences, took more than a generation, and continues today. The ideas were implicitly as much philosophical as technical and scientific, although philosophic issues were not given their due (except at the first meeting) because the desire was for purely scientific discourse.

Although according to the title of the conference series the biological and social sciences were to be highlighted, it turned out differently than one might have expected in regard to the social sciences. Concepts brought by the mathematicians (game theory and cybernetics) were presented as specifically useful for economics and political science.[10] Yet no economist or political scientist was among the regular participants, nor even among the one-time guests. On the other hand, psychology and psychiatry were heavily represented in the group. The fields of anthropology and, to a lesser extent, sociology were at least represented among the conferees, and the subject of linguistics was brought in with the help of one-time guests. The bias for psychology and psychiatry over economics and political science as representative of the social sciences was in part a manifestation of the aforementioned social atomism and retreat from politics popular at mid-century, and in part indicates that even the interests of the cyberneticians lay in the first instance in mind and brain. Both the mechanical bias and the psychological bias reflect an optimistic belief in our power to explain things human and social in explicit terms. They also represent, however, a retreat from the original intention to bring in all the social sciences.

The mode of discourse at the meetings after the first was intended to be neutral-scientific and apolitical. Discussions of political science and economics, unlike psychology and engineering, were more likely to lead to loaded political issues. The Macy group safely stuck to "scientific" topics, and its invited speakers were not of the kind to bring leftist politics into the discussion. The mechanical and psychological (atomistic) biases served to depoliticize the issues.

Just because fundamental philosophical and political theory controversies were not pursued does not mean that all was harmony at the Macy meetings. In fact strong differences in view concerning practices in psychiatry, the mental health movement, the validity of the mathematical utility function, the psychology of perception, and many other topics were aired at the meetings. There was much to talk and argue about. Political controversy appeared in off-the-record conversations and correspondence among participants.

We shall briefly adumbrate the substantive content of the first morning and afternoon of the meetings, entirely taken up with presentations by the cyberneticians. Human scientists were audience that could interrupt to ask questions. McCulloch was chairman.

The morning of March 8 was devoted to a description of the workings of general-purpose electronic digital computers by John von Neumann, the leading designer in the country of the logical structure of these yet-to-be-built computers,[11] and certain analogies to the "computing machine of the nervous system" described by Rafael Lorente de Nó, who was at the time engaged in ingenious experiments revealing the electrical properties of nerve cells.[12]

Encouraged by the conceptual and logical demonstration of the possibility (in principle) of creating a general automaton (Turing machine)[13] that can carry out any operation that can be unambiguously and completely described in a finite number of words, von Neumann had been working under wartime pressure with engineers and mathematicians since mid-1944 to create an actual general-purpose computer. His presentation included discussion of the greater precision of digital machines as compared to the older analog computers, the use of binary rather than decimal representation of numbers, the stored program concept, various methods available for storing and accessing information, and how in detail arithmetic operations are carried out by these machines. Some methods could not be discussed because they were still classified as military secrets. Von Neumann made semi-quantitative comparisons between vacuum tubes and neurons, the overall size of brains and computers, their speed of operation and other characteristics. Just as von Neumann described the exemplification of an automaton in the metal, so Lorente de Nó spoke about neurons as elements of an automaton in the flesh. The individual neuron consists of a cell body and one or more axons along which an electrochemical impulse can be propagated. Impulses arriving via axons from other neurons stimulate or in some instances inhibit a neuron from firing an impulse along its own axon. But the impulse, whenever it occurs, always has the same strength. Thus the firing of an impulse from a nerve cell can be conceived as a digital, binary process: A stimulus either generates an impulse or it does not. This fact is usually referred to as the all-or-none character of nervous activity. Like a piece of electronic equipment, the various characteristics of a neuron can be described quantitatively: A definite threshold voltage is required to stimulate a discharge; a certain "delay time" separates the arriving and the departing impulses; the impact of two arriving impulses will supplement each other provided they arrive within a well-defined, short time-span, the so-called period of latent addition; and so on.

The background of the Neumann-Lorente de Nó presentation was that in the early 1940s McCulloch and Pitts had proved that if one constructs a simplified mathematical model of a neuron and links a sufficiently large number of such neurons into a nervous net, that net defines a formal universal automaton. Essentially, the automaton can carry out all operations that can be specified in a finite number of words (the precise definition of the automaton is highly technical). It was essential for their result that impulses can travel in closed loops. In 1943, when their paper first appeared, interest in the subject was so low that they feared it would go unnoticed and felt lucky that it got published at all.[14] The presentations of von Neumann and Lorente de Nó combined to make vivid the McCulloch-Pitts result and gave some cogency to its likely relevance for the group hearing about it for the first time. The McCulloch-Pitts model suggested a way of approaching mind and brain with the help of formal logic, neurophysiology, and engineering (their ideas will be described further in the following chapter). It was certainly appropriate that psychologists and psychiatrists, experts on the human mind, were at the meeting to hear about these notions and developments. Within a short span of years the computer, the McCulloch-Pitts model, and other information processing models for minds and brains would inform the practice of many working in these fields.

The Neumann-Lorente de Nó presentations led to a few comments pointing to limitations and posing questions. Wiener noted that if a computer is asked to solve a Russellian paradox (e.g., "This statement within the parentheses is false"), it will go into a series of oscillations flipping from "This is true" to "This is false" without settling down. What do human brains do when confronted with paradoxical situations? Gerard and Bremer emphasized aspects of brains ignored in the Pitts-McCulloch picture: Chemical concentrations and continuous electric fields are known to play a role, but do not fit the digital all-or-none model. Just how important are these continuous physiological variables in human thought processes?

Another mathematician-physiologist team spoke in the afternoon. Wiener and Rosenblueth developed more fully the notion of purposive activity with negative feedback and the consequences when the feedback mechanism breaks down, Wiener focusing on the design of machines and Rosenblueth on homeostatic mechanisms and, more generally, on purposive behavior in organisms.[15] Among the biological examples Rosenblueth described were the process of respiration, nystagmus (an eye condition), clonus (periodic contraction of an overstimulated muscle), cerebellar tremors (which occur when feedback is not adequately damped), and the automatic maintenance of steady blood pressure and steady temperatures. In considering design, Wiener described how a machine may have the equivalent of receptor and effector organs, and contain a computer and various other electronic circuits to function effectively, taking cognizance of the world around it as needed to pursue its goal successfully. He reviewed the history of automata: Numerous toys invented by Heron of Alexander in ancient Greece, such as the "Automatic Wine Dispenser, Controlled by the Rising and Sinking of a Float", utilized feedback mechanisms.[16] He described in some detail the centrifugal governor used by

James Watt in the late eighteenth century for regulating the speed of a steam engine by negative feedback. After describing mechanisms for self-regulation in machines, and general principles applicable to them, he observed that the points of most theoretical and practical interest concern communications: "The fundamental idea is the message, even though the message may not be sent by man and the fundamental element of the message is the decision." Wiener introduced the group to fundamental ideas from what have now become known as information theory and communication theory.

On the first day von Neumann and Wiener did not presume to tell the social scientists about social science. By the end of the second day, however, von Neumann gave them an introduction to the theory of games, which he had, with an economist, applied to economics.[17] And within a few months of the meeting, the chairman sent around to all participants a long, discursive article by Wiener containing an inventory of his many ideas, including the basic notions of his theory of information. He proposed to link statistical mechanics, communication engineering, the theory of control mechanisms in machines, biology, *and also psychology and social science* by the common theme of "communication," and gave texture to his general argument with the help of concrete examples. His position was that

> the neuromuscular mechanism of an animal or of man is certainly a communication instrument, as are the sense organs which receive external impulses. Fundamentally the social sciences are the study of the means of communication between man and man or, more generally, in a community of any sort of being. The unifying idea of these diverse disciplines is the MESSAGE, and not any special apparatus acting on messages.[18]

That first day was significant in that von Neumann, Wiener, Rosenblueth, McCulloch, and Lorente de Nó attempted to bridge the gap between themselves and the human scientists and signaled their availability for continued conversation about the new notions they had presented. The mathematicians, physiologists, and engineers had shown those in the human sciences a tool-box with a variety of general-purpose conceptual tools. Characteristically, the new concepts spanned the human and the inanimate, leading to mechanical metaphors for human characteristics and anthropomorphic descriptions of machines. Communications and cognitive processes were central foci of interest; the new notions would impinge on traditional theories of cognition and language. The concept of circular causality was adopted immediately in referring to goal-directed systems with negative feedback and stretched to encompass even the circular currents in the McCulloch-Pitts model. It suggested formulations of greater complexity and subtlety than traditional causal theories, but retained the scientific predictability inherent in those theories. In traditional thinking since the ancient Greeks a cause A results in an effect B. With circular causality A and B are mutually cause and effect of each other. Moreover, not only does A affect B but through B acts back on itself. The circular causality concept seemed appropriate for much in the human sciences. It meant that A cannot do things to B without being itself effected.

This book describes how these new currents of thought were received and adapted by the group sitting around the table at the Macy Conference at the Beekman Hotel on 8–9 March, 1946, a particular, small sample of those in America working in the human sciences. While many of the concepts had originated during the war, researchers in the human sciences first came to terms with them during the postwar years and the Cold War era, and it is not surprising that the social and political conditions of that period influenced what transpired. The process occurring throughout the series of ten conferences displayed a microcosm of diverse concerns and intellectual commitments and how these new concepts came into conflict or confluence with existing disciplinary traditions. As indicated more explicitly below, the human scientists presented some of their thoughts, research results, and concerns on the second day, 9 March 1946.

The cast of characters in this intellectual drama includes, first of all, *Warren McCulloch,* chairman of the series of ten conferences. He was at the time a professor of psychiatry and clinical professor of physiology at the University of Illinois, but in October 1952 he joined the research staff at the Research Laboratory of Electronics at M.I.T. *Walter Pitts,* his young collaborator both in Chicago and at M.I.T., was a polymath, but primarily, perhaps, a mathematician. At the conference McCulloch himself explained how signals in a net of neurons can duplicate the calculus of propositions in logic. He discussed in general terms two distinct levels of description of communication processes: the purely physical (the electrical currents and acoustic vibrations in a telephone connection), and the significant content of the message (the conversation on the phone). He suggested that impulses traveling in closed loops in the brain constitute one form of memory and are in a sense timeless, whereas most other processes occurring in a net happen at a particular time.

Gregory Bateson, an anthropologist, after a year on a Guggenheim Fellowship, taught briefly at the New School for Social Research (1947) and Harvard (1948), then became a lecturer in anthropology at the Langley Porter Psychiatric Clinic in San Francisco (1949), but soon became head of a research group at the Veterans Administration Hospital in Palo Alto, California, where he remained for many years. At the March meeting Bateson described, jointly with *Margaret Mead,* what they saw to be the lack of adequate theory in the social sciences and what was required in the way of theory. Mead permitted Bateson to take the lead in the presentation. He illustrated his comments by referring to his own observations in various cultures, including the case of the Iatmul culture in which a transvestite ceremony served as a homeostatic mechanism whenever a characteristic pattern of aggressive actions within the tribe threatened to divide them. He also distinguished ordinary learning from learning to learn, and stirred discussion by asking whether computers can learn to learn, and how in a formal mathematical way one could distinguish that from plain learning. Mead was throughout the seven years of the conference series the assistant curator of ethnology at the American Museum of Natural History in New York.

Filmer S. C. Northrop was a professor of philosophy at Yale University, the only professional philosopher at the conference. At the meeting he raised the question of the possibility of deriving ethics from the natural sciences. But he regarded social sciences as ordinarily different from the natural sciences, especially when the former entail normative theories. In the natural sciences the theory adjusts to facts. But in the case of normative social theories it is the other way around. The society, Northrop claimed, will adjust itself to the governing normative theory. The challenge, as Northrop saw it, was to construct a valid normative theory based on proper science. His aspiration was viewed with skepticism by other conferees.

Lawrence K. Frank, author of books in psychology and social science, had at one time been, but was no longer, a vice-president of the Macy Foundation. While a foundation executive, he had significantly influenced the direction of the social sciences in the United States. On 9 March he observed that the new interdisciplinary concepts presented at the conference needed for their clarification and refinement a new kind of language with a higher order of generality than is customarily used in any one discipline. *Frank Fremont-Smith,* who at the time ran the foundation's conference program, had come from a medical research background. He opened the conference with the announcement, "Each group, when it comes together, is an experiment. If it excites you all enough to want to meet again, we will plan for further meetings." On the second day the group agreed to meet again in October. Consideration of these two participants, Fremont-Smith and Frank, illuminates the relations of the human sciences to philanthropic foundations, such as the Macy Foundation, and their policies.

Molly Harrower was an authority on psychological tests of personality, such as the Rorschach (inkblot) test, and the vagaries of human perception generally. On 9 March she spoke about the systematic differences of perception between people with anatomically damaged brains and normal individuals.[19] At the time of the 1946 conference she was a research associate in neuropsychiatry at the University of Wisconsin but moved in 1952 to become research director of the Manhattan Child Center at the University of Texas. *Lawrence Kubie*'s presentation followed Harrower's. He defined neurosis, emphasizing the characteristic compulsive repetition of unsuccessful behavior by the neurotic person, and discussed reasons for the prevalence of sexual maladjustment in American society. Walter Pitts expressed that he was "extremely much interested" in the concept of psychological energy and its transformation and asked probing questions about the origin and modifiability of the repetitive patterns of the neurotic, whereas Wiener questioned Kubie about using the notions of "psychic tension" or "energy" because Kubie seemed to him to be speaking of a system in which communication is central and "information," not energy, would be the crucial variable. Kubie was on the faculty of the department of psychiatry and mental hygiene at the Yale University School of Medicine and a practicing psychoanalyst in New York City.

Heinrich Klüver was a psychologist at the University of Chicago. He presented examples of experiments showing that through feedback mechanisms, human perception of objects and the external world generally so adjusts itself as to be approximately constant regardless of which senses are used, what the point of view is, or what one's position is relative to the object. These mechanisms allow one to judge with some reliability the relative size, weight, and positions of objects. He called his interest the constancy of the *milieu externe,* playing on the better-known emphasis (from Claude Bernard to Rosenblueth) on the *milieu interne.* At the meeting he also pointed to an area of ignorance: Psychology, he said, has no adequate theory as to what biologically determines how a brain perceives forms (*Gestalten*) and how a human knows what a brain perceives. He was in effect posing a concrete problem to challenge the advocates of computerlike models of the brain.

Kurt Lewin, a social psychologist with a considerable following, was at M.I.T., where he had created the Research Center for Group Dynamics. He was also engaged in "action research" with the American Jewish Congress. He tended to speak rapidly and defer to others when they interrupted him, so that from the noisy mechanical recording device we unfortunately have no record of the substance of any comments he made at the first meeting. At the second meeting, however, he gave a long discussion of concepts from Gestalt psychology and social psychology. Another participant was *Paul Lazarsfeld,* a sociologist and the director of the Bureau of Applied Social Research at Columbia University. At the March 1946 meeting he proposed that a separate get-together be organized for those especially interested in social science. He wanted to bring in some sociological theorists and introduce them to the new concepts.

Leonard J. Savage was a young mathematician interested in statistics, who was working on problems in biology and economics. He went to the University of Chicago as a Rockefeller Fellow in 1946, and remained there (becoming a faculty member in the department of statistics) throughout those years when the ten Macy conferences were held.

This completes the list of the social and psychological scientists at the 8–9 March meeting, a substantial majority among those present. All of those listed will appear in the following pages, although some will receive more attention than others. The remaining participants of that meeting will be largely ignored here. They were John von Neumann and Norbert Wiener, who are the subject of a previous book, and Julian

Bigelow, an engineer who had worked with both of them;[20] and the group of neurobiologists, G. von Bonin, R. Gerard, R. Lorente de Nó, and A. Rosenblueth, who could easily be the subject of another book. The issues in neurobiology are rather different from those in the social sciences. Finally, the freshwater ecologist G. E. Hutchinson is not treated in the present book either, although I was tempted to add a chapter on him (he has written an autobiographical book[21]). Some additional people, all listed in the appendix, joined the group, either as regulars or as occasional guests, at subsequent meetings. A few of them will appear as they come into the story.

The theoretical formulations in the human sciences acceptable in that period of history, as at any period, are not independent of the social and political circumstances at the time, even if a glad postwar sentiment of some natural scientists at meetings early in 1946 was, "Here we are, ready, willing, and able to talk over some questions which are of great importance, but definitely nonpolitical."[22] The circumstances, sketched in chapter 1, provided the broader context for the selection of participants at the conferences, for the focus of their discussion, as well as their general attitudes. Controversial social theory, especially if it entailed socialist ideas, did not appear at the conferences any more than it did in academia generally. No historian or political scientist was ever invited, even as a guest, and the sole sociologist participating was safely interested only in statistical methods. With the exception of the first meeting, explicit philosophical discussion was muted. The ideal of purely scientific discourse dominated all the meetings after the first. Whereas mechanism was an underlying motif, a theme popular after the technological successes in connection with the Second World War, the very existence of human feelings (so subjective!) was consistently played down or explained away over the protest of a few of the participants. Even such anthropocentric social scientists as Mead and Frank became proponents for the mechanical level of understanding, wherein life is described as an entropy-reducing device and humans characterized as servomechanisms, their minds as computers, and social conflicts by mathematical game theory. The analogies between automata and servomechanisms and human thought and actions sanctioned the adoption of mechanical metaphors, which in turn fostered thinking of oneself and one's community as mechanical systems. For those with social concerns who believed they could do something to improve the human condition or alleviate and prevent misery, their action was likely to be formulated in terms of models suggested by repairing or adjusting a complex mechanism ("system"), be it an individual or a society. Some would, in their optimism, suggest the benefits of a management of society by social science experts. But this left open the question of how such expert management was to be reconciled with popular democratic politics. The notion of circular causality put an egalitarian gloss on managerial aspirations.

The ideas promulgated at the conference series became themselves a kernel around which optimism snowballed. As one of the guest participants (Bar-Hillel) recalled, cybernetics and information theory "created among many of us the feeling that the new synthesis heralded in them was destined to open new vistas on everything human and to help solve many of the disturbing open problem concerning man and humanity."[23]

The high hopes placed on science and technology, the great technological optimism prevalent around 1950, made some uncomfortable. Wiener, who was irrepressible in his enthusiasm for the scientific ideas presented at the conferences, published a book, *Cybernetics: Control and Communication in the Animal and the Machine,* in 1948, which included a fair amount of the mathematical background as well as the diverse topics from the human sciences discussed at the meetings. The book became a best-seller at the time and has become a classic. In spite of his enthusiasm Wiener articulated some misgivings. "Those of us who have contributed to the new science of cybernetics," he wrote, "stand in a moral position which is, to say the least, not very comfortable. We have contributed to the initiation of a new science which, as I have said, embraces technical developments with great possibilities for good and for evil."[24]

Aside from his misgivings about extensions to technology, Wiener was skeptical about the possibilities of using cybernetics in sociology, anthropology, and economics. In spite of the urgency of the social problems, which Bateson and Mead emphasized and Wiener acknowledged, he did not share "their hopefulness that sufficient progress can be registered in this direction to have an appreciable therapeutic effect in the present diseases of society."[25] Referring to what he viewed as the "false hopes" that some of his friends entertained for the application of the new ideas to anthropology, sociology, and economics, he maintained that they "show an excessive optimism, and a misunderstanding of the nature of all scientific achievement."[26] Bateson was troubled but not deterred by Wiener's opinion.

At the seventh meeting Ralph Gerard suggested that the popular attention given to Wiener's book may in itself have contributed to the overoptimism. He expressed irritation about the publicity the meetings had received, after the book appeared in print, "in extensive articles in such well-known 'scientific' magazines as *Time, Newsweek,* and *Life.*" He continued,

> It seems to me, in looking back over the history of the group, that we started our discussion in the "as if" spirit. Everyone was delighted to express any idea that came in his mind, whether it seemed silly or certain or merely a stimulating guess that would affect someone else. We explored possibilities for all sorts of "ifs." Then, rather sharply it seemed to me, we began to talk in an "is" idiom. We were saying much the same things, but now saying them as if they were so. I remembered a definition of pregnancy: "The result of taking seriously something poked at one in fun," and wondered if we had become pregnant and were in some danger of premature delivery.[27]

He goes on to give a historical example of overoptimism and "premature delivery": "In the early 1800s a flood of mathematical articles based upon the teaching of phrenology and exploiting them quantitatively, issued from the best minds of the time."

Both Wiener's and Gerard's cautionary responses to the unbridled confidence and optimism protected them from the disappointment with cybernetics that some, such as the enthusiastic Bar-Hillel, experienced later.

This story of the social scientists at the Macy meetings is of necessity incomplete and selective. It is a foray into some of the human sciences after the Second World War, a not very developed area of historical study, intended to encourage further inquiry.[28]

At the same time, this book is an attempt to approach an event in the history of science in such a way that informal contacts, conversations, and consensus among groups of practitioners are taken seriously as belonging to the history of scientific developments, even as the substantive results and methods of research are taken seriously.

For purposes of exposition I have chosen two people for special attention, one from the cyberneticians and the other from the social science cluster. For both of them the conferences were pivotal events, and both became lifelong exponents of the ideas presented there. McCulloch is the protagonist from the cyberneticians: Not only was he chairman of all of the meetings, but as a trained psychiatrist his interest in psychology and psychiatry was profound, and he was outspoken in the discussion concerning many of the issues arising from the social sciences. Among the social scientists none made more imaginative use of what he learned at the conferences than Bateson, and—even though much has been written about him since he and his work first came to interest me—he is the second principal figure. He, too, entered into the dialogue on nearly every topic, sometimes with a perceptive question and at other times with an assertion.

It is noteworthy that both Bateson and McCulloch became marginal to the mainstream of their professions, as well as institutionally, although interest in Bateson's work did revive in the 1970s and in McCulloch's in the 1980s. Both of their styles were exceptionally open and freewheeling. Of course, these two individuals represent only themselves, and cannot be taken as typical of either the cyberneticians or the social science cluster. But if one had to select only two, these two are, after Wiener and von Neumann, the most pivotal figures for the history of cybernetics.

3

Describing "Embodiments of Mind": McCulloch and His Cohorts

How do the people most deeply committed to scientific investigation use ideas and concepts? Albert Einstein spoke for many when he described the motives for scientific studies:

> Man seeks to form for himself, in whatever manner is suitable for him, a simplified and lucid image of our world, and so to overcome the world of experience by striving to replace it to some extent by this image. This is what the painter does, and the poet, the speculative philosopher, the natural scientists, each in his own way. Into this image and its formation he places the center of gravity of his emotional life, in order to attain the peace and serenity that he cannot find within the narrow confines of swirling personal experience.[1]

What kinds of images or abstract constructions will satisfy those whose studies are centered on the human and the social? One wonders whether images of people based on automata and communications engineering could be so congenial as to be conducive to peace and serenity. Such mechanical schemes—at their most concrete, images of ourselves as complicated robots—seem to ignore the flux of our immediate experience, our sense of freedom, the pulse of life, deeper meanings, as well as personal and human feelings, and are antipathetic because they dehumanize us. They seem to invite us to think of others as mere objects to be manipulated or to give scientific legitimation to those who operate on that premise. One might think that technical research along these lines would stimulate the development of new technologies that would foster dehumanization, exploitation and oppression.[2] But are all these concerns merely expressions of unwarranted timidity?

Our story begins with Warren Sturgis McCulloch, whose work and character show these questions to be simplistic. He was born in Orange, New Jersey, on 16 November 1898, the son of James W. McCulloch and his young second wife Mary Hughes (Bradley) McCulloch. James McCulloch was a self-made businessman, manager of a large estate with holdings in railroads and mining enterprises. Warren's mother came from a Southern family. She was strongly religious, taught bible history, and was involved in Episcopalian church affairs. The McCulloch household in Orange included Warren's considerably older half-brother and his sister, Margaret, a year younger than Warren. The summers were often spent on Nantucket Island off Cape Cod, where Warren learned to sail and as a boy enjoyed the adventurous world of ships and whaling captains—leaving him with a lasting feeling for the sea.

In 1917 he entered a Quaker College, Haverford, with the intention of entering the ministry in accordance with his family's wishes, but his interests turned to philosophy and mathematics. One family member at least, Warren's sister, took up religious interests and became an active Quaker and pacifist. Warren sought understanding of who we are in mechanical rather than religious terms. In his words,

> At last we are learning to admit ignorance, suspend judgement, and forego the *explicatio ignoti per ignotium*—"God"—which has proved as futile as it is profane. Instead we seek mechanisms . . .[3]

The seeking of mechanisms in the brain to describe how cognitive functions are carried out eventually became the central theme in McCulloch's work. His interests in the thought and theology of the medieval schoolmen, however, persisted throughout his life.[4] Probably no other neurophysiologist in the twentieth century talked so much about the views of St. Bonaventura, Duns Scotus, William of Occam, or Peter Abelard—men of the twelfth and thirteenth centuries. He himself resembled the medieval scientists rather than most modern ones, in that his science was "an integral part of a philosophical outlook."[5]

After graduate work in psychology (Columbia University M.A., 1923) during which he learned of the behaviorist, psychoanalytic, and introspective schools in the field, he remained unsatisfied and went to medical school. He then interned in neurology and worked in mental institutions, still with the intent of learning what he would need. In the depth of the depression (1932) McCulloch took a job in the Admissions Service of Rockland State Hospital. He found an intellectual comrade there in Eilhard von Domarus from Holland, who was engaged in an original work intended to form the basis for a "scientific psychiatry," philosophically grounded in Aristotle, Hegel, Husserl, and Yale's Northrop. McCulloch worked with von Domarus in formulating the latter's thesis in good English.[6] Both men were concerned with logical structure of mind and its relation to neurophysiology and to madness. Domarus saw it as scientifically untenable for "the science of alienation from society and the study of physiological psychology to progress, in the main, independently." He had paid particular attention to language, whereas McCulloch emphasized the biological entities, neurons. Domarus, whom McCulloch later invited to attend the one meeting of the cybernetics group that focused on language, had pioneered the notion that the speech of schizophrenics obeys laws other than those of adult logic, and had spelled out the laws of logic he deemed characteristic of schizophrenics' language.[7]

If we accept McCulloch's elegant autobiographical account,[8] he was animated by a philosophical question that would continue to motive him, once having accepted Bertrand Russell's concept of number: "What is a man that he may know a number?" Coming from philosophy, he was concerned, as he recalled, "with the problem of how a thing like mathematics could ever arise—what sort of a thing it was. For that reason, I gradually shifted into psychology and thence, for the reason that I again and again failed to find the significant variables, I was forced into neurophysiology."[9]

McCulloch earned his credentials as solid citizen in the world of brain research by the mainstream experimental study of chimpanzee and monkey brains he conducted during the 1930s in the laboratory of Dusser de Barenne at Yale University Medical School. Chimpanzees' and monkeys' brains are sufficiently similar to human ones that their detailed study leads at least to hypotheses concerning human brains. In particular, McCulloch studied the cerebral cortex, the gray matter forming the outermost layer of the cerebral hemispheres, which plays a prominent role in connection with the most subtle and complex mental functions. In a typical experiment McCulloch and his coworkers activated a specific region of the cortex by giving it an electric shock or applying strychnine to it. The local stimulation caused an electrical pulse to travel from one nerve cell or group of nerve cells to the next, generating an itinerary characteristic of the particular point of stimulation. They monitored the spread of the stimulus by placing recording electrodes at various points on the surface of the cerebral cortex. The purpose of this and other experiments was to map out the "functional pathways" in the cortex—the routes by which impulses actually travel in the brain. Although a number of interesting rules of behavior of impulses within the brain were derived from this research, and some innovation in experimental techniques was possible, on the whole it was systematic, detailed, tedious work aimed at generating a fund of empirical information on which neurobiologists could draw. It attests to McCulloch's standing as a leading expert on the subject that in 1944 he was chosen to write the major article reviewing the whole field of "the functional organization of the cerebral cortex."[10]

Although highly individualistic, McCulloch was never a loner. Talk with colleagues was indispensible to him. While a researcher at Yale, he joined various seminar groups and was an active participant in a philosophical seminar for research scientists in which Filmer Northrop provided the professional philosopher's perspective. Northrop recalled that

> One evening at a meeting of this Yale scientific research group, the symbolic logician, Frederic B. Fitch, gave a descriptive report of the primitive concepts and postulates of the theory of deduction and mathematical calculation in Whitehead and Russell's *Principia Mathematica*. . . . Upon the presentation by Fitch, McCulloch urged Fitch to work on the symbolic logical formulation of neural nets and attended advanced lectures by Fitch on certain logical operators.[11]

The seminar provided McCulloch with a good opportunity to discuss his intellectual agenda, expose it to philosophical critique, and translate it into a seemingly practicable scientific research program. He knew that "number" could be defined by means of the logical system elaborated in *Principia Mathematica*. In that work mathematics is shown to be only a special instance of general logic. A logical proposition, McCulloch noted, is either true or false, and correspondingly a nerve cell when stimulated either produces an electrochemical discharge across its synapse or it does not. It is like an all-or-none proposition. Furthermore, neurons are linked to each other in the cerebral cortex, so that one neuron's firing leads to the firing of others in a chain, analogously to how logical propositions are linked, the truth of one implying that of another. If such a correspondence between logic and nets of neurons in the cortex could be formally established, one could view the neural nets as functionally equivalent to a large general-purpose logical reasoning machine or a computer. Laboratory studies of the functional organization of the nervous system would be a step toward an experimental epistemology. Through the combined use of rigorous formal logic and careful neurophysiological experiments one could presumably learn scientifically to know how we know numbers and much more, and express this understanding in terms of mechanism.

Such a research program providing knowledge of cognitive processes was not only scientifically but also philosophically exciting: It gave explicit form for testing the question, Is such knowledge of a different type than other scientific knowledge, say how the heart works, because it is reflexive, in the sense of mind knowing itself? It would fulfill some of the thinking of seventeenth- and eighteenth-century philosophers, especially some of Leibniz's ideas about knowing and perceiving developed in his *Monadology,* which McCulloch was reading at the time.[12] Kant's notion of synthetic *a priori* knowledge would be given a material basis, as both Northrop and Dusser de Barenne appreciated. It was also in the tradition of Descartes' efforts in the experimental study of the nervous system to inform epistemology, turning a philosophical problem into a scientific one.[13] At the same time exploration along these lines could provide a scientific description encompassing both sides of the dichotomy of mind and body, thereby apparently overcoming Cartesian dualism. McCulloch envisioned such a research program but lacked the considerable mathematical and logical prowess needed to make progress with it. Nor did Fitch at the time see how to do it properly.[14]

The inner-directed, individualistic Warren McCulloch was on his own kind of scientific-epistemological quest for understanding mind and brain. Although he could and did identify a long and honorable lineage of intellectual predecessors with an outlook similar to his own, he was out of step with the predominant thinking in universities and research centers in the thirties and early forties, as his interest in thinking mind was congenial to neither behavioristic nor psychoanalytic schools of thought. (At the time it was congenial to Rashevsky's small group in mathematical biophysics at the University of Chicago, as well as to Northrop. It became widely acceptable, however, only after the Second World War, as prototype high-speed electronic computers were being designed and built.) In fact some of McCulloch's public self-descriptions suggest that he was animated by a sense of being a daring pioneer and sometimes thought of himself as akin to the heroes of mythology and legend, who courageously set out on a journey to obtain a boon to benefit mankind. His journey was an intellectual one, the holy grail was a formal logical description of the functioning of the brain, explicating mind in terms of mechanism, and the boon would be of the nature of knowledge. He wrote:

> Even Clerk Maxwell, who wanted nothing more than to know the relation between thoughts and the molecular motions of the brain, cut short his query with the memorable phrase, "but does not the way to it lie through the very den of the metaphysician, strewn with the bones of former explorers and abhorred by every man of science?" Let us peacefully answer the first half of this question "Yes," the second half "No," and then proceed serenely. Our adventure is actually a great heresy.[15]

McCulloch was careful to point out that he was interested in those facets of mind that are amenable to description in terms of rigorous logic and neurophysiology, and he gladly left out of consideration whatever else may constitute human mind and soul. He sought to push mechanism to describe embodiments of mind, so as to demystify knowing, as far as possible, and separate genuine mysteries from what is scientifically knowable. In his own words, one of his objectives was to "exorcise ghosts" from the description of mind and replace them with mechanistic hypotheses sufficiently specific to be tested experimentally. Although his own ability and training as a logician were limited, he sought the crystal clarity of abstract logic and mechanism. He admired brilliant logical minds. As an experimenter in the laboratory, as well as on the farm near New Haven where he lived, he respected and had an excellent rapport with the instruments, tools, and machinery with which he worked.

In a contentious discussion of the metaphor of the machine, McCulloch on one occasion, when he was along in years, said rather sharply: "I don't particularly like people, never have.

Man to my mind is about the nastiest, most destructive of all the animals. I don't see any reason, if he can evolve machines that can have more fun than he himself can, why they shouldn't take over, enslave us, quite happily. They might have a lot more fun, invent better games than we ever did."[16] Whatever his personal disappointments with people may have been, man's inhumanity to man, in small ways and large, is and was no secret.

On other occasions over the years McCulloch spoke of a feeling (love) for machines, comments that puzzled anthropocentric humanists and disturbed psychoanalysts. He was furthermore convinced that machines can be designed that "suffer emotions."[17] What is clear is that for McCulloch, who was at home repairing, using, building machinery, neither the den of the metaphysician nor mechanical images held any terror. In fact, for him such images may have been conducive to the peace and serenity that Einstein referred to in speaking of the motives for research.

Yet even the above quotation indicates the high value McCulloch put on the capacity to play and to "have fun." Those who knew him spoke of his enormous sense of delight, fun, joy. Although games can be described and the act of playing analyzed, these are distinct from the fun of it. As Huizinga said, "The *fun* of playing resists all analysis, all logical interpretation. As a concept, it cannot be reduced to any other mental category."[18] McCulloch's scientific preoccupation was with organic mechanisms rather than with experimental phenomena. When focusing on that level of explanation, it is easy to fall prey to the fallacy of misplaced concreteness and forget that no description of mechanism informs us about the nature of "having fun." But no matter, McCulloch turned the machine into a "subject" and—at least in 1968—readily attributed to some electronic devices the experience of having fun. He was intent on "humanizing the machine," as he put it.

At the time of the Macy meetings, although already inclined to humanize the machine, McCulloch had—in a lecture in 1948—in passing allowed that humans are distinguished from robot-automatons (those in existence at that time), first by the process of individual human development in interaction with the environment, and second by the human "joy" of creating ideals.[19] On other occasions he would deny or minimize that distinction, as in his 1952 lecture on ethical robots. Having noted von Neumann's theory of self-reproducing automata, and that a machine can be made capable of learning and thus improve upon itself, McCulloch suggested that

> it is possible to look on Man himself as a product of . . . an evolutionary process of developing robots, begotten of simpler robots, back to the primordial slime; and I look upon his ethical conduct as something to be interpreted in terms of the circuit action of this Man in his environment—a Turing machine with only two feedbacks determined, a desire to play and a desire to win.[20]

Because his style of expression is literary rather than scientific, and terms are not precisely defined, we may see McCulloch's assertions about machines as poetic expressions of his sensibilities in relation to artifacts. Nevertheless, he buttresses his attitudes by reasoned philosophical arguments. Over the years during which the Macy group met, McCulloch seemed to become increasingly confident of his mechanistic views.

Formal logical systems such as the Pitts-McCulloch model, as well as machines that form a concrete instance of such systems, are subject to Gödel's Incompleteness Theorem and other metamathematical theorems indicating inherent limitations. To the Macy participants acquainted with mathematical logic, it was something of an open question whether and how these limitations would manifest themselves as more elaborate and detailed attempts were made to describe the human mind and brain. It was becoming clear to them, however, that the multitude of paradoxes and contradictions pervading and enlivening human thought do not invalidate the notion that machinelike neuronal circuits are its physical substructure.[21]

McCulloch was tall, thin, narrow-shouldered, and loose-limbed. His head was long, with high cheekbones. He had a sizeable beard, unusual in those days, which changed from black to gray to white during the 1940s, and elongated features reminiscent of El Greco's paintings. In conversation he, more than most scientists, looked directly at the person talking with him, and his whole face became animated, his intense, blue eyes lighting up in discussion of technical points of any scientific problem or idea that caught his imagination. There were many of these ideas, for he was intellectually open and alive. His intellectual vitality and enthusiasm combined with a verbal gift, an enjoyment of scientific talk, and a personal informality and warmth. This, together with his strong sense of commitment, gave him a charisma that attracted many a bright young student. He was the most loose and spontaneous, least machinelike of men.

In personal relations to his favorite students, as to his friends, he was generous and sensitive. Many an impecunious young scientist was helped by him financially; his hospitality was such that over the years a large number of young people stayed at his house for short or long periods of time; and then he went to considerable lengths to make suitable professional contacts for these young scientists. Over and above practical help, McCulloch showed personal interest in their well-being and their freedom to pursue their ideas or develop their talents. He allowed himself to become involved with them. As one who had been helped by McCulloch in his youth commented, many professors are kind to students and younger colleagues, but often a young man needs more than kindness: McCulloch took sufficient interest to perceive and respond directly to the particular needs of individual young scientists and to help them effectively. Another friend, a woman, put it this way: "Warren was in a way a very selfless person and genuinely devoted to those people he had rapport with." Another comments: "I owe a great deal, both in the sheer personal hospitality he offered and in the contacts that he made for my work. I know a lot of chaps who wouldn't have got where they are, if it weren't for Warren. . . . He is one of the best human beings for goodness to young people that I've ever come across; he is amazingly self-sacrificing." Among the young people he brought to the Macy cybernetics conferences were John Stroud, Donald McKay, and Heinz von Förster. To each of them he extended warm friendship and help. For instance, McCulloch was instrumental in enabling von Förster to emigrate from Austria, introducing him professionally in America and arranging a position for him. As McCulloch's colleague Henry Brosin wrote to him, "Your support of younger men is magnificent."[22] There was in McCulloch's way much tenderness, concern for people, and enjoyment of friendships.

Clearly, McCulloch does not fit any stereotype of a cold and compulsive mechanist, nor of one obsessed with mastery and efficiency. He was a man with a strong desire to understand human mind and logical thought in terms equally vivid, tangible, explicit, and lucid as his comprehension of the repairable workings of his automobile. His romanticizing the machine was a touch of mysticism in his make-up, but his liking for things mechanical was squarely in the American tradition in which he grew up.

In 1941 McCulloch left Yale and took a position at the University of Illinois Medical School as director of research for a group of about thirty researchers in the department of psychiatry. He saw the group's mission as an attempt to lay the biological foundations for a scientific approach to psychiatry, although in fact the group's work was more diversified. It was a decisive moment for McCulloch when he encountered a teenage run-away from Detroit who had sought refuge in the academic environment of the University of Chicago, where he hoped to be understood. The combination of McCulloch's longing for the crystal clarity of logic and his personal kindness brought about his association with Walter Pitts.

Walter H. Pitts Jr. was the second of three sons born into a blue-collar Detroit family on 23 April 1923. When only fifteen, he was studying symbolic logic with Carnap, the foremost philosopher in Chicago, and mathematical biology with its leading practitioner, Rashevsky. A couple of years later this precocious adolescent met McCulloch.[23] The McCullochs, characteristically, took into their home this homeless, needy, shy, eccentric prodigy. In Pitts, Warren found a brilliant coworker with such powerful ability in science and especially in logic that together they could proceed productively in the research McCulloch had so long envisioned. Many an evening and early morning was spent at the McCulloch's house with Warren McCulloch, Walter Pitts, and young colleagues talking, talking, talking. Warren would smoke one cigarette after another and frequently refill his glass from the bottle of Scotch. To most fellow scientists the tenor of the talk would have seemed strange, for the group was concerned with fundamental philosophical issues of metaphysics and epistemology. For these men the spring for research activity was as much philosophic as scientific.

Walter Pitts was simultaneously an extraordinarily talented, sought-after scientist and an adolescent boy, with all the incongruity that entails. Other leading scientists, even when they took a fatherly interest in the boy, invariably responded to Pitts the powerful scientist with admiration. Norbert Wiener's evaluation of Pitts was typical:

> He is without question the strongest young scientist whom I have ever met. . . . I should be extremely astonished if he does not prove to be

one of the two or three most important scientists of his generation, not merely in America but in the world at large . . . he has as a scientist magnificent equipment.[24]

The McCulloch-Pitts theorem for nervous nets, mentioned earlier, deserves a fuller description. It was the apex of the scientific achievement of both men. It required remarkable scientific abstraction to go from the pinkish-gray tissue of the brain and its known electrical, chemical, and anatomical properties to the construction of formal neural nets isomorphic to the relations of propositional logic. Since the work carried out in Spain by Ramon y Cajal (a line of research subsequently elaborated by Lorente de Nó in Madrid and, later, in the United States) it had been firmly established that the central nervous system consists of distinct cells, neurons, separated from each other by a membrane and a "synaptic gap," and that each neuron includes a long fiber, the axon, that conducts electrical pulses away from the cell to which it belongs. The axon divides into several branches, each of which ends by nearly touching another neuron, creating a synapse at the point of near contact. The fiber arriving at a synapse can be excitatory or inhibitory (fig. 1). If and only if the net excitation of a neuron during the brief period of latent addition exceed the neuron's threshold voltage will transmission take place across the synapse and a pulse be generated and travel from the neuron along its axon toward other neurons.

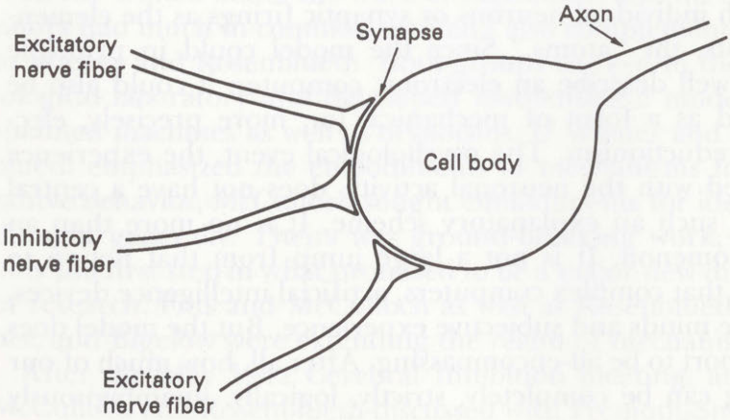

Figure 1 Diagram of neuron

To see how neurons might represent logical assertions, consider two examples. Suppose that both A and B are excitatory fibers, and that a pulse traveling along either suffices to cross the synaptic gap. This physical situation corresponds to the formal statement, "If either A or B is true, then C is true." A different logical statement corresponds to the situation where fiber A excites the neuron at regular intervals, but fiber B is inhibitory: "Assuming A is always true, C is true whenever B is *not* true." The Pitts-McCulloch model consists of a net made up of such idealized neurons, each with some number of excitatory and inhibitory inputs, a threshold voltage, and some branching of its output. Finally, the time scale is introduced. The essential result of their analysis has been summarized as follows: "That anything that can be completely and unambiguously described, anything that can be completely and unambiguously put into words, is ipso facto realizable by a suitable finite neural network . . ."[25] While the theorem showed that a large class of mental operations *could* be carried out even in a highly simplified model of the brain, it was and remains largely unknown how the brain carries out cognitive functions. The proof, a tour de force by Pitts, nevertheless held out the promise that eventually the functioning of the brain, including pathologies, might be understood by combining logical analysis with detailed experimental neurophysiology. The grandness and generality of the theorem cried out for finding concrete, specific, testable applications.

As a mode of explaining psychological events, the Pitts-McCulloch approach represents a kind of organic reductionism, with individual neurons or synaptic firings as the elementary units, the "atoms." Since the model could in principle equally well describe an electronic computer, it could also be regarded as a form of mechanical (or, more precisely, electronic) reductionism. The psychological event, the experience associated with the neuronal activity, does not have a central place in such an explanatory scheme. It is no more than an epiphenomenon. It is not a large jump from that notion to imagine that complex computers, artificial intelligence devices, also have minds and subjective experience. But the model does not purport to be all-encompassing. After all, how much of our knowing can be completely, strictly, logically, unambiguously spelled out?

The Pitts-McCulloch approach to mind did not have much common ground with the prevailing schools of psychology and psychiatry. Its focus on mind and brain was irrelevant from a behaviorist's perspective; its emphasis on propositional logic and on the organic was of little interest to psychoanalysts; its neglect of the process of an individual brain's evolution from infancy to adulthood made it at best marginal for developmental psychology; its atomistic reductionism seemed in conflict with the premises underlying Gestalt psychology; and its focus on mechanism rather than subjective experience put it at odds with the phenomenologists. But McCulloch and Pitts did have overlapping interests with those physiological psychologists and neuropsychiatrists who studied human and animal behavior in terms of the physiology and anatomy of the nervous system. They had a still closer intellectual kinship with the young British psychologist Kenneth Craik, who independently of the American researchers had proposed that "in a neural calculating machine there may well be patterns of excitation in the cortex, temporal and spatial groupings of impulses, and so forth which, to a physiologist sufficiently skilled, would 'represent' concepts or sensations of objects. . . ."[26]

Craik, in 1943, had in a little book presented the case for the organic-mechanistic mode of explanation to which McCulloch and Pitts were devoted. Craik inspired others in England, who eventually formed an active group of researchers. In 1944 Craik became the head of the unit for Research in Applied Psychology at Cambridge University, but died as a result of a bicycle accident the following year.

In the United States the work of McCulloch and his collaborators had much in common with and also complemented that of Wiener and Rosenblueth. Both groups worked in the physiological laboratory and developed mathematical models that spanned machines as well as organisms. If Wiener and Rosenblueth emphasized the embodiments of mechanisms for purposive behavior, McCulloch sought embodiments for ideas and thought generally. Theirs was ground-breaking work, clearly only the first step in what promised to be a major new direction of research. Pitts and McCulloch as well as Rosenblueth, Wiener, and Bigelow were extending the realm of mechanism.

After the May 1942 Cerebral Inhibition meeting, at which McCulloch and Rosenblueth discussed with Fremont-Smith the possibility of a postwar meeting, the two teams made common cause and sought out opportunities for dialogue. Pitts was sent to M.I.T. to work with Wiener for a year and stayed longer but traveled back and forth. McCulloch tried to bring Arturo Rosenblueth to Chicago, when Rosenblueth lost his job at Harvard, but his Mexican citizenship turned out to be an obstacle. But both Pitts and Wiener traveled repeatedly to Mexico City to work with Rosenblueth. Wiener's daughter, Barbara, a student of neurophysiology, spent some time with McCulloch's team in Chicago, but McCulloch and Wiener, working at M.I.T., visited each other frequently. The extended yet intimate scientific family of Rosenblueth-McCulloch-Wiener-Pitts also included a couple of other scientists, only a few years older and special friends of Pitts, who, however, were not invited to the Macy conferences.[27] Furthermore, Wiener was in contact with the mathematician John von Neumann, then, in response to military urgency, engaged in computer design, who found the Pitts-McCulloch model of nervous nets suitable for describing the logical structure of general-purpose computers, where its functioning could be seen in the metal. Automata theory put computers and neural net models under one heading. Von Neumann at Princeton joined the M.I.T.–Mexico City and Chicago teams to form an impressive core group for making a place within American science for the new machine-organism analogies.

Pitts's talents were of a particular kind. He was primarily self-educated. He was known to master the contents of a textbook in a field new to him in a few days. When he was only twenty, his detailed, precise, and comprehensive knowledge and understanding of mathematical logic, mathematics, physiology, and physiological psychology were already on a par with those of leading practitioners in each of those fields.[28] He was also studying and digesting the thinking of major Western philosophers, appreciating subtle features of their thought, and carrying on "conversations" with them in which McCulloch and friends could serve as third or fourth parties. Moreover, his logical reasoning was impeccably clear and precise, and his attention to detail assiduous. These talents made him an ideal collaborator and critic for someone like Wiener or McCulloch, and the latter especially came to depend on him. For if their ideas, reasoning, and assertions survived Pitts's scrutiny, they could rest easy that no error of logic, no misinformation, had entered their work, that they were guilty of no omission or neglected perspective. Otherwise Pitts would have set the matter straight.[29] He held more diverse individuals' complex thoughts in his mind than most of us would imagine possible. He could also use his power as a logician or mathematician, manipulating symbols, to carry through such new and difficult arguments as that contained in his 1943 paper with McCulloch. Wiener, who also had great mathematical power and an excellent memory in matters scientific, differed from Pitts in that originality in science and mathematics was for him a strong suit. In turn, he did not give Pitts's kind of detailed, in a sense, self-effacing, attention to others' work.

A passive, personally needy, unworldly young genius like Pitts was vulnerable to those who, unlike his own father, appreciated, admired, and came to depend on his remarkable mind. If one were to try to place his collaborative research with McCulloch within the context of current discussions of the relationship of gender to science, such as those by Evelyn Keller, I think one would have to say that it more nearly echoes the world of Plato (science as a sublime love affair with the "essential nature of things") than Bacon's vision of science as power. It also carries the homoerotic associations of Plato's world, in which science is often a joint venture of an older and a younger man, with in this case the latter embodying exquisitely the logical (masculine) nature of mind. As in Plato's vision, the unconsummated eroticism is transmuted into intellectual energy.

Pitts's friends, including McCulloch's daughter Taffy, enjoyed his company. He was playful in a scientific, intellectual way, inventing all kinds of word games. He liked to go camping with a friend or by himself. He used chemicals to concoct dyes, fireworks, and pharmaceuticals in the McCulloch basement. His talk was always impersonal, and he never mentioned his family to anyone. He was a slim, shy, gentle, unobtrusive young man. A woman with a young child lived in the McCulloch house for a time and found in Walter Pitts a dependable babysitter. His face reminded at least one person of a frog. His gentleness with the young contrasted with the sharpness of his intellectual arguments and show of contempt toward sloppy reasoning by social scientists. It became blatantly evident in the 1950s that he spurned any interest in his own career, the imperative to write papers, institutional or bureaucratic requirements of any kind. He inhabited a different, purer world. He had something in common with those of his age-cohorts who dubbed themselves the Beat Generation and were put off by the prevailing compulsive "return to normalcy" mood of career and material well-being following the Second World War. He, like the Beats, spent a good bit of time "on the road." On one occasion he set off (with Oliver Selfridge) in a vintage second-hand Cadillac hearse from Boston for Mexico City. They stopped in the Colorado mountains for a while, backpacking. The hearse gave out for good in San Antonio, Texas. But the greatest consternation among their elders was not that the boys would arrive late or not at all, but that the Gibbs analyzer, an expensive piece of scientific equipment they were delivering to Rosenblueth, would be stranded in San Antonio as well. When in 1955 McCulloch and Jerome Wiesner successfully arranged to give Pitts a Ph.D. from M.I.T. on the basis of his knowledge and work, though he had never taken a single course for credit or fulfilled formal Ph.D. requirements, Pitts refused even to sign the piece of paper—the one act required from him to receive the degree! Pitts seemed stubbornly to seek anonymity, even as Norbert Wiener, Warren McCulloch, Jerome Wiesner, and others tried over the years to rescue him from it.

But unlike the Beats, Pitts was not drawn to concern with feelings or love relationships. His primary world was the mental universe of science and mathematical symbols. In spite of closeness to and affection for a few friends with similar intellectual interests, he stayed away from the sensual. His fear of women, his antisensuality, his inability to respond in kind to overtures of personal friendship made by some Macy participants, all make him appear eccentric. In the late fifties he withdrew increasingly from all his M.I.T. friends who had come to depend heavily on him and eventually avoided nearly all contact with them.[30] Some of the seemingly unusual attitudes represented by McCulloch and Pitts are echoed in the current generation of inventive artificial intelligence buffs and computer hackers at M.I.T.[31] Consequently, McCulloch and Pitts may be regarded as members, nay founders, of a whole clan, rather than only as highly idiosyncratic individuals.

McCulloch was to a considerable extent sympathetic to Pitts's ways, as the moral of the following story, told in 1961, of scientists of ancient Greece suggests:

> The citizens of Abdera wrote to Hippocrates crying for help because their great atomic scientist had gone mad. Hippocrates was long delayed. When he arrived with his bottle of hellebore, the weeping citi-

zens led him to Democritus, where he sat unshod, dissecting animals and making notes in the book on his knees. Hippocrates asked why he was doing it, and he answered that he was looking for the causes of madness in the parts of beasts, and he demanded what had detained Hippocrates. He answered, "Family matters, engagements, money, and other business." Democritus roared with laughter—that men called great so waste their lives, marrying only to fall out of love, seeking wealth without measure, making wars to no purpose, and in peace overthrowing one tyrant to set up another. Hippocrates listened to his railing and, turning to the people, told them to cease their lamentations, for Democritus was not only sane but the wisest man in Abdera.[32]

Aspiring to the sanity of Democritus, McCulloch sought after about a decade to shed his academic status and administrative duties in Chicago and, despite a salary cut, to join his young friends—Walter Pitts and Jerome Lettvin—so as to engage happily with them in research in Cambridge, Massachusetts. They became employees of M.I.T., where the three were given laboratory space in Building 20, a temporary structure thrown up during the war and administered by the Research Laboratory for Electronics.

The 1942 conference on hypnosis and the conditioned reflex had established a connection between neurobiologists and human scientists preliminary to the cybernetics meetings, but the development of the cluster we have labeled "the cyberneticians," which already crossed many disciplinary boundaries, is a different story. McCulloch had long known Lorente de Nó and admired his work, but the latter was by temperament somewhat less gregarious than the others. In the 1930s Lorente and Rosenblueth had been on opposite sides of the scientific debate about the primacy of electrical processes as opposed to that of the production of chemicals in the transmission of nerve impulses, but by the end of the war, "Behold! the whole subject has become suddenly clearer."[33] McCulloch and Rosenblueth had been professionally and personally close. Both had briefly worked in clinical psychiatry but had eagerly turned away from it to neurobiological research. They visited each other and sought opportunities to talk and do experiments together. Wiener and von Neumann had had an analogous relationship throughout the 1930s. They admired each other's work. Both treasured opportunities for conversation and visited each other for days at a time to talk.[34] All of them had with high expectations worked together to coordinate their presentation of material to kick off whatever might develop from the 8–9 March 1946 meeting. As Wiener wrote to McCulloch,

> This meeting is going to be a big thing for us and our cause. I am now down with von Neumann discussing plans and I can assure you that his part and mine will be well coordinated. Pitts and I are also getting busy together and so is Rosenblueth. . . . We enjoyed having your daughter up very much and hope to see more of her next year. Meanwhile, we are impatient for the meeting of the Macy Foundation and when we shall see you and talk over many things of common interest. I am very much pleased with the tentative program and I am delighted that you are chairman.[35]

In the introduction to *Cybernetics,* Wiener described the background of the March 1946 Macy conference from his perspective. In the thirties he and Rosenblueth had already taken part in a supper club devoted to discussion of topics of common interest, especially topics related to scientific method. He and Rosenblueth had by the 1930s agreed about the need for interdisciplinary work and a team of scientists from various disciplines to explore "some of the blank spaces in the map of science. . . ." In that connection they had "dreamed for years of an institution of independent scientists, working together in one of these backwoods of science. . . ." By the mid-forties these dreams had taken concrete form around the subject of cybernetics. McCulloch recalled his first encounter with Wiener: "I first met him at dinner with Rosenblueth when they, with Bigelow, were mechanizing teleology. He told me promptly what I could expect of my own theories of the working of the brain. Time proved him right. Then it was that the dream began of team play between biologist, mathematician and communication engineer, which eventually flowered into cybernetics. . . ."[36]

Pitts, because of his youth, represented the promise for the future in this field. If he was in effect McCulloch's adopted son, his relationship to Wiener was also close and personal. From Wiener, too, Pitts elicited a paternal attitude. Wiener, having been raised by a father who imposed harsh intellectual discipline, was inclined to demand of Pitts as well a high standard, more work and less play. McCulloch would patiently defend Pitts's ways to the more demanding and sometimes critical Wiener. But when Pitts was depressed and lonesome in New York, Wiener made the occasion to come from Boston to New York and visit Pitts with the intention of cheering him up.[37] Sometimes Wiener and McCulloch appeared to compete for Pitts's collaboration.

A complication arose in 1943, the very year in which the seminal papers in the field appeared. Rosenblueth, after fourteen years at Harvard, was informed he would lose his position in the following year.[38] At Harvard he was personally disliked by some and had experienced social prejudice because of his Mexican-Jewish ancestry. His department head, Walter Cannon, wanted to keep him at Harvard because of his great merits as a scientist. He defended Rosenblueth, as seemed unfortunately necessary, to a colleague:

> You ask about Dr. Rosenblueth as a man. Let me tell you about him. . . . Dr. Rosenblueth has a Jewish name but his Jewish ancestry is remote. He has none of the unpleasant characteristics sometimes associated with the Jew. One of his sisters is a nun, and he is married to a charming American, a graduate of Reed college in Portland, Oregon, and for some years a graduate student at Radcliffe College.[39]

But it was of no avail. Rosenblueth's collaboration with Wiener had begun, and its future was now in jeopardy. In spite of the possibility of returning to Mexico to start a research institute from scratch, Rosenblueth preferred a position at an already thriving scientific center in the United States. McCulloch managed to arrange for an associate professorship, which would at first be temporary but would carry the assurance of eventual permanence, at the University of Illinois.[40] To buy equipment for Rosenblueth's laboratory from Harvard and elsewhere, McCulloch contacted Fremont-Smith about obtaining funds from the Macy Foundation. Rosenblueth had already accepted the offer when he learned that he would have to give up his Mexican citizenship to be eligible for the tenured position. Thereupon he backed out. McCulloch wrote to Fremont-Smith,

> To my sorrow, Rosenblueth is not coming to us but returning to Mexico. We were unable to promise him permanent tenure if he retained his Mexican citizenship, which he, for patriotic reasons, decided to retain. . . . I shall be delighted to be with you for supper and spend the evening of Friday, December 10th, and if I may will pick you up at the airport at 5:02 p.m. that afternoon.[41]

In Mexico City Rosenblueth was to build up and head a department of physiology of a new research institute. From 1946 on the Rockefeller Foundation supported Rosenblueth in Mexico financially, as part of its policy to promote science in Latin America. Robert Morison of the Rockefeller Foundation had been a friend of Rosenblueth and a member of the Harvard supper club at which Wiener and Rosenblueth had first met. Rosenblueth's move to Mexico and his acceptance of new organizational and administrative responsibilities did not prevent his dialogue and collaboration with Wiener, McCulloch, Pitts, and their friends. All of them and von Neumann visited Rosenblueth in Mexico City. Wiener made a formal arrangement permitting him to spend six months in alternate years with Rosenblueth in Mexico, engaging in collaborative research. The Rockefeller Foundation supported Wiener's visits to Mexico, but not Rosenblueth's visits to M.I.T., since it wanted to keep Rosenblueth in Mexico.[42] As the heavy correspondence among them shows, close contact among members of the cluster of cyberneticians remained intact in spite of Rosenblueth's move. It is also clear that the assist from foundations (Macy, Rockefeller) and the good will of foundation executives (Fremont-Smith, Robert Morison) were indispensible.

In the midst of wartime the idea of a new field for scientific research, along the lines that several years later came to be known as cybernetics, was taking shape in informal conversations. Von Neumann and Wiener agreed on a plan for a meeting of a small group of men "to discuss questions of common interest and make plans for the future development of this field of effort, which as yet is not even named."[43] It was at the resulting meeting in January 1945 that a strong consensus was formed. Rosenblueth could not attend because he was busy getting things started in Mexico, but Wiener reported to him that the meeting

> was a great success. I believe you have already got von Neumann's report. . . . The first day von Neumann spoke on computing machines and I spoke on communication engineering. The second day Lorente de Nó and McCulloch joined forces for a very convincing presentation of the present status of the problem of the organization of the brain. In the end we were all convinced that the subject embracing both the engineering and neurology aspect is essentially one, and we should go ahead with plans to embody these ideas in a permanent program of research . . . we definitely do have the intention

> of organizing a society and a journal after the war, and founding at Tech or elsewhere in the country a center of research in our new field. . . . When this scheme really gets going, I for one will not be content unless we can bring you and Bigelow directly into it.[44]

That meeting established a consensus concerning a research program among the senior "cyberneticians," and efforts to implement it followed.[45] Von Neumann was confident he could get financial backing, and he and Wiener received indications of support from Warren Weaver of the Rockefeller and H. A. Moe of the Guggenheim foundations. Von Neumann believed that in connection with finding a home for the center he and Wiener were envisioning, "the best way to get "something" done is to propagandize everybody who is a reasonable potential support."[46] Wiener in particular sought to arrange a center at M.I.T. and especially to bring von Neumann, a big name, first. Indeed, M.I.T. made von Neumann a good offer, and Wiener could write to Rosenblueth, "Johnny was down here the last two days. He is almost hooked."[47] In the end, however, the Institute for Advanced Study, in Princeton, gave von Neumann, who had plans for building a prototype computer, what he wanted, and he did not go to M.I.T. Wiener recommended Bigelow, the engineer with whom he had worked during the war, to von Neumann, who then made Bigelow the engineer for the new Princeton computer.

The "center" did not materialize; Rosenblueth was in Mexico, von Neumann and Bigelow in Princeton, Wiener at M.I.T., Lorente de Nó at the Rockefeller Institute in New York, McCulloch in Chicago, and Pitts going back and forth between M.I.T. and Chicago. McCulloch's going to see Fremont-Smith after the war to suggest the Circular Causal and Feedback Mechanisms in Biological and Social Systems conference was another expression of the cyberneticians' effort to establish and extend the new field of research and arrange opportunities to talk with each other. McCulloch as "chronic chairman" of the series of conferences was in a position to control to a considerable extent who would be invited, who would be asked to present a paper, and who would be given the floor when the discussion got heated. The adaptation of the cyberneticians' ideas to the human sciences, however, was contingent on the response and prior outlook of groups and individuals within those fields.

4

Raindancer, Scout, and Talking Chief

In describing the great scientific revolution of the seventeenth century, some historians have restricted themselves to a purely internal "history of ideas," while others have focused on particularly brilliant figures such as Isaac Newton and Robert Boyle. Some scholars have put the emphasis on the political, economic, religious, and industrial circumstances peculiar to seventeenth-century England, while feminists have concentrated on the prevailing views of male and female. Some in their studies have given prominence to small, elite groups, networks, and organizations, especially the Royal Society and the informal group known as the "invisible college" that preceded it. The "invisible college" collected around a nonscientist, Samuel Hartlib, "whose principal occupation was the promotion of schemes, mainly educational or religious, for the public good."[1] One can look at an intellectual movement through diverse lenses, each bringing into focus one or another significant feature of what *in toto* may be an intricate and elaborate story.

However simple the notion, we have spoken of the "human science cluster" that dominated the Macy meetings. Its structure has parallels and differences from that of the cluster we labeled "the cyberneticians." Casting about for other analogies in the history of science, the seventeenth-century "invisible college" is a natural one to use. Lawrence Frank's role could then be likened to that of Hartlib, Gregory Bateson's to that of one of the major scientists particularly strong in theory construction (though he was also a careful observer), and Margaret Mead's to another major scientist whose forte was empirical research and actively promoting the Royal Society itself. To make an analogy to seventeenth-century British science is in effect to claim that such clusters serving to evaluate research and guide and promote particular directions are not peculiar to the participants at the Macy meetings on cybernetics, nor even to the practice of twentieth-century science. Sometimes research results have been dismissed as of no interest because no supportive cluster of scientists valued them; at a later period the same results are hailed by a prestigious group of scientists as a marvelous discovery. A famous instance of this is Gregor Mendel's discovery in the 1860s of factors in heredity today called genes, and the rediscovery of his work in 1900, when it first entered the mainstream of science thanks to its exposition and promotion by, among others, Gregory Bateson's father.

The notion of a cluster does not suggest any differentiation in the style and function of its individual members, as the parallel to the invisible college does. For our purposes, better than either concept is to view the human science cluster as a tribe, analogous to those that Mead and Bateson had studied in their anthropological research. Certain important members of the tribe served specific functions. In this chapter I will suggest that Bateson's role was that of "scout," Frank's that of "raindancer," and Mead that of "talking chief." All three were part of the inner circle or "tribal council." I have spoken of a core group among the cyberneticians as an extended family or clan, as they were close both intellectually and personally. The threesome in the human sciences were still closer and are appropriately viewed as an extended family. In 1946 Bateson and Mead were man and wife. They were living in a downstairs apartment on Perry Street in Greenwich Village in a house owned by Larry Frank. Frank and his wife and children lived upstairs. For Catherine Bateson—the young daughter of Gregory and Margaret—the Franks became part of her extended family and provided a more stable home than her itinerant parents could. Mead and Bateson divorced in 1950. Mead continued to make the Perry Street apartment her home base after Bateson had moved to California. As seen through Catherine Bateson's eyes, all three were her "parents." From Mead's perspective, the other two members of the tribal council were presumably also the two most important men in her life, although she had already been married twice before meeting Bateson. The network, however, extended far beyond the core family to a whole tribe.

Larry Frank served as a highly effective liaison between philanthropic foundations and researchers. He has not so far been the subject of a biography, as have both Bateson and Mead, and we shall allot considerable space to him in this chapter. His story is one in which a foundation executive, who after all is answerable only to his board of trustees, successfully prevailed on universities to shift the direction of their research along lines he advocated.

One direct way the Frank-Mead-Bateson trio influenced the composition of the Macy group on cybernetics was to get their friends (Kurt Lewin, Alex Bavelas, Dorothy Lee, Evelyn Hutchinson, Erik Erikson, Clyde Kluckhohn) invited. Behaviorists were relatively poorly represented, even though, working with conditioning experiments, they were more closely linked to neurophysiology, the primary interest of many at the cybernetics meetings. In fact it was Bateson, with Mead and Frank behind him, who had been responsible for the inclusion of social scientists at the cybernetics meetings in the first place.

When I first encountered Gregory Bateson, by reading the transactions of the Macy meetings with a view of possibly doing a historical study, I wrote him before contacting most of the others because I imagined from his comments that I would find him a most congenial person. His response to my letter was enthusiastic: "There is certainly a piece of scientific history to be dug out of these meetings—I believe more profound and dramatic than *The Double Helix*"[2] (as I learned, he believed that the reductionist approach of molecular biologists gave only superficial and misleading ideas about the nature of life). Bateson offered to stop off on his trip from Hawaii to Europe to visit with me in Detroit, where I was then living. His interest in the project helped firm my tentative intention to make a fuller study of the cybernetics meetings.

Bateson's life can rightly be described as an intellectual odyssey, but his quest was not so specific as McCulloch's. As a naturalist given to observation, he at first lacked theoretical tools and formulations. But he found these, partly through cybernetics, and evolved an epistemology and a stance toward life and the world that went beyond the conventional Western scientific world view. Like McCulloch, he moved from field to field and for each specific research project sought "to put on blinders" in concentrating on the details as—according to Max Weber—is necessary for the practice of the scientific vocation. His learning was cumulative, and his interest was always in illuminating general, transdisciplinary issues.

Gregory Bateson was born on 9 May 1904, near Cambridge, England. His father, William Bateson, was already then a major, albeit controversial, figure within British biology, looked upon with special favor by Cambridge University because his father—Gregory's grandfather—had been a master at St. John's College. William Bateson nevertheless had difficulty finding adequate funding for his botanical researches. For years he earned a modest income as a steward at St. John's, administering the college's kitchens. After a brief period as holder of an endowed chair at Cambridge, which provided status and honor but neither research funds nor a large salary, William Bateson in 1910 resigned his professorship to head a newly founded horticultural institute outside of London.

William Bateson's powerful personality and intellectual interests dominated the household in which Gregory, the youngest of three sons, grew up. From his extensive observations and breeding experiments with plants and animals, William Bateson had come to conclude that biological variations do not form a continuum, but take place in discrete steps. This contention embroiled him in passionate scientific controversy with the biometricians, especially Karl Pearson. The rediscovery of Mendel's work, in 1900, gave Bateson substantial ammunition for his point of view, and he became the leading exponent of Mendelian genetics in the British Isles.[3] His interests extended beyond biology. He was a close friend of Alfred North Whitehead, and he had a special affinity and fondness for the drawings and poetry of William Blake. Gregory's mother was the father's diligent helpmeet in the work with plants and animals and seemed to go along with his general views and attitudes. She kept a journal, wrote a short biography of her husband, and maintained a correspondence with each of the boys when they were in college.

Gregory Bateson's early scientific education took place spontaneously under the tutelage of his two older brothers as well as his father. He learned naturalistic observation early in connection with the pleasure of exploring with them many kinds of life in the woods and meadows of his environs. And in his home he heard of fascinating controversies surrounding comprehensive theories of the origin of the large diversity in nature, especially the theories of Lamarck, Charles Darwin, and Samuel Butler. He picked up from his father an interest in order of an abstract kind, such as the perpetuation and creation of bilateral symmetry, pentagic symmetry, radial symmetry or asymmetry in plants and animals, and those similarities between two organisms that descended from a common ancestor.[4] Gregory learned to notice when the formal relationship between some elements in one organism was the same as that between entirely different elements in a very different organism. He appreciated that a similar formal relationship between elements can link apparently unlike natural phenomena.

As a student at St. John's College of Cambridge University he chose zoology for his major subject. The pattern of education of scientists at Cambridge encouraged broad, general interdisciplinary interests, which has been manifested since by a number of students aside from Bateson: C. H. Waddington, Joseph Needham, J. D. Bernal, and Evelyn Hutchinson. Marxist thought offered the most fully developed social theory, and Needham, Bernal, Haldane, Blacket, and many other young Cambridge scientific intellectuals came to adopt it, but Bateson preferred a different, "simplified and lucid image of our world," one in which neither history, economics, nor politics was given a prominent place, but ecological pattern was central. In 1925, at the age of twenty-one, he decided to leave zoology, explaining to his parents that zoology is "a purely impersonal science," and that he was shifting to "anthropology which I think would supply the personal inspiration which I believe myself to need."[5] His father gave his blessings to Gregory's step away from his own domain. In his work in anthropology Gregory carried over the habit of careful naturalistic observation on the one hand and the concern with abstract patterns on the other. However important the personal, he continued to function as a scientist. One of his brothers had died in combat in the First World War, and the other had shot himself soon thereafter; it became incumbent on Gregory to carry on, in some form, the tradition created by his father. The violent deaths of his brothers must have harshly disrupted the pattern of Gregory's world.[6] It may have contributed to his pessimism about ameliorating human suffering through political revolution or politics generally.[7] He later spoke often of his anger or pain at the disruption of patterns.

Gregory's first major research in anthropology was a study of the Iatmul tribe in New Guinea.[8] From his fieldwork he concluded that an Iatmul village is nearly perpetually threatened by fission of the community because it is characteristic that intense and growing rivalries occur between two groups, and it puzzled Bateson that usually the community does not disintegrate. He found that one important event heading off a blow-up is the elaborate "Naven" ceremony, which entails transvestism and buffoonery. To analyze the dynamics of the culture, Bateson introduced the new concept of "schismogenesis," which can have either a "symmetrical" or a "complementary" form. In symmetrical schismogenesis a competitive pattern of interaction between two groups (or individuals) continuously exacerbates the rivalry (for example, through boasting and provocative challenge), eliciting an ever more specialized behavior in members of each group toward those of the other—a situation that is necessarily highly unstable. In complementary schismogenesis the two parties interact by each specializing in opposite ways: for example, one is increasingly dominant and the other increasingly submissive, or one exhibitionist and the other admiring. Such a pattern can also run amuck. The specific topic of research in both types of schismogenesis is the mechanism serving to exacerbate or restrain the escalation, but in more general terms it is "the reaction of individuals to the reactions of other individuals."[9]

Bateson cast a wide net for the concept of schismogenesis: It may occur in the relationship between a husband and wife and lead to divorce; in the realm of psychopathology, schismogenic relations of a person to those nearest to him or her may lead to "growth of the symptoms of the paranoid individual"; the response of parents may be such as to promote their childrens' tantrums; the politics of international rivalries, and especially arms races, are a form of symmetrical schismogenesis, while the class conflict, with its economic basis—as emphasized by Marx—is, according to Bateson, of the complementary kind. The tension of schismogenesis does not inevitably have destructive consequences, as the naven ceremony illustrates. It can also lead to a love-feast in which the tensions are dissolved. Ordinary life, for most of us, entails some schismogenesis and its attendant tensions. When Bateson and Margaret Mead studied Balinese culture, they found it strange because nearly all kinds of schismogenesis were absent, and it was just the observed lack of intensity in personal interactions that called for explanation.[10]

It is noteworthy that Bateson's use of the concept of schismogenesis ranged from psychiatry to international relations. In each case a particular pattern of interaction can be observed, and he could establish that indeed it fit the abstract concept. He never saw the abstract concept as equivalent to any actual concrete circumstance, which is typically a rich, complex human story; Bateson, who often spoke of Whitehead's "fallacy of misplaced concreteness," took care not to confuse the two. Naturalistic observer that he was, his interest in the qualities of the actual personal and interpersonal goings-on (which for the cultures he studied he attempted to convey with the help of film, photos, and words) was primary. The abstractions served to shed further light on the interpersonal events and to link them to other events, but he found them nevertheless unsatisfactorily vague.[11]

In 1941, at the Symposium on Science, Philosophy and Religion, Bateson had introduced the notions of proto-learning and deutero-learning.[12] Psychologists had earlier distinguished in their experiments the learning proper of nonsense syllables and the subject's increased speed at such a task with successive trials, that is "learning to learn." Bateson's interest was in a broader question along the same line—how we learn apperceptive habits. Do we see an action in terms of a goal or in terms of its intrinsic value? Do we see it in terms of hope or despair concerning the future? Do we see it in the frame of human autonomy or compulsion? He distinguished learning of apperceptive habits (deutero-learning), determining the contexts within which we place events, from ordinary learning of skills or information (proto-learning). These abstract definitions encompassed not only the learning theories emanating from psychology laboratories but apperceptive styles of different cultures, and even contemporary quasi-political issues such as contrasting learning in totalitarian with democratic contexts.

In short, Bateson had developed a few taxonomic notions, although he considered them somewhat too vague, which nevertheless were useful for discourse within the social sciences. Like some observations of symmetries in biology, these notions did not constitute a theory, and Bateson was troubled by the lack of adequate theory. His view of existing theory in the social sciences was that the traditional functional analysis of British anthropology "was not likely to lead anywhere,"[13] that

Marxian theories only overemphasize one particular complementary schismogenesis,[14] and that psychiatric thinking ought to pay more attention to interaction between individuals.[15] A sense of the near-bankruptcy of extant theories in the social sciences made him all the more receptive to new formulations. He was in the 1940s hopeful that the mathematicians and engineers at the cybernetics conferences could provide tools for better theory construction in the social sciences. At the first (March 1946) meeting he and Mead described what they saw as the requirements for theory in the social sciences. Nor were his expectations from the cyberneticians disappointed, judging from what he wrote many years later: "The two most important historical events in my life were the Treaty of Versailles and the discovery of Cybernetics."[16]

It is not surprising that Bateson found the new ideas stimulating, especially those presented by Wiener, for they seemed to be close to his own earlier notions, but more precise, more general, and demonstrable and analyzable in mathematical models or electromechanical gadgets. Thus positive feedback was akin to schismogenesis, and negative feedback its counterpart leading to stability, but in the machine one knows precisely the mechanism, so that analogy invites looking for details of the process in the human situation. The Russellian Logical Types, used as heuristic analogy, could provide an abstract framework not only to contain proto-learning and deutero-learning but to deal with all communications and even provide a way of encompassing indeterminacy and paradox in the overall picture. And the emphasis on communications was congenial to Bateson's interest in interpersonal interactions. Yet Bateson felt constrained, when he confronted the concepts of cybernetics in 1946, to spell out carefully the epistemological basis and implications of transferring the ideas from mathematics and engineering to social studies.

Larry Frank's origins, as well as his relation to the practice of social science, were entirely different from Bateson's. At the time of the cybernetics meetings Frank was an ex-foundation executive and a freelancing conference goer, "educator, mental hygienist, author."[17] He had as well taken over the responsibility for a center that offered evening classes on child development to teachers, which had been started by school psychologist Carolyn Zachry, and at the cybernetics meetings he listed as his professional affiliation the Carolyn Zachry Institute for Human Development. At the last few meetings he simply listed his home address.

Frank was born 6 December 1890 to a well-to-do family in Cincinnati, Ohio. His father's brother had a large business dealing with groceries. Larry was six when his parents separated, and he was henceforth raised by his mother and maternal grandmother. For a time they were quite poor, and the absence of substantial help from his father during times of hardship rankled. Eventually he moved to New York City with his mother, who opened a boarding house in Greenwich Village. After high school he studied at Columbia University, majoring in economics. Already as a student he took on demographic work with the Bureau of Social Research in the city and was impressed with the rate of infants' and mothers' mortality among poor families; he also had occasion to investigate child labor in canneries. When he was in his twenties, he encountered and learned from two capable women: the first was Frances Perkins, with whom he worked for a year on municipal issues in New York. Perkins later became President Roosevelt's Secretary of Labor. The other, Lucy Sprague Mitchell, formerly a college dean, implemented and probably originated the concept of combining an experimental school for children with a research organization studying child development. Larry Frank worked with her when she founded the first "laboratory school," later known as the Bank Street School and the Bank Street College of Education in New York. The theme of combining research and practice in child development was one of Frank's active interests until the end of his life.[18] Lucy Mitchell's husband, Wesley Clair Mitchell, an economist who had studied with both Thorsten Veblen and John Dewey, and who was known for his theory of business cycles, became a good friend and mentor.

After college Frank worked for some years as a supervisor and manager for the New York Telephone Company and then for a short time became the business manager for the New School for Social Research, where he derived some stimulation from the thought of Veblen and Dewey, who were on the faculty. But he came most fully into his own during the years from 1923 to 1936, while he was successively on the staff of the Laura Spelman Rockefeller Memorial (1923–1930), the Spelman Fund (1930–1931) and the Rockefeller General Education

Board (1932–1936). It is for his activities during that time period that he is remembered by historians today. In his foundation work his humanitarian concerns and increasing acquaintance with the world of economics converged. An excerpt from an historical account gives a good overview of how Frank used his interests and abilities to essentially start a new field of research. Beardsley Ruml, head of the Laura Spelman Rockefeller Memorial, in spring 1923 asked Frank to suggest how they might spend approximately a million dollars a year for the benefit of children.

> Frank responded with a basic outline of what became the parent education movement: a program of child study for mothers gathered in small groups and based on scientific research in child development, to be implemented by sponsorship of university-based research centers, fellowships for training scientists and practitioners, and parent organizations to supervise mothers on the local level. Frank's proposal was more astute, for it both retained and updated the Memorial's original mandate. Ruml gave Frank the go-ahead to develop the idea on his own. . . . Having committed himself to science as the key to social progress, to radical educational innovation as the key to liberating "intelligence," and to the early childhood years as the key to molding healthy personalities, he was able to envision a social movement which, starting at rock-bottom with child-rearing practices in the home, would radiate outward and eventually transform all social institutions. As Dewey was the apostle of the "progressive" school, Frank became the apostle of the "progressive" home.
>
> . . . Mainly through Frank's initiative, the nation's first well-funded, university-based research on children—the Institute of Child Welfare Research—opened late in 1924 (at Columbia University's Teachers College). . . . In short order LSRM placed Iowa's Child Welfare Research Station on equal financial footing with Teachers College's, and during the next few years other institutes followed at Berkeley, Toronto, and Minnesota. . . . Frank successfully used large financial inducements to direct social scientific research in directions he considered "progressive". . . . To help parent education realize its full potential as a vehicle for revolutionizing American institutions . . . he proposed to make a small but unusually sophisticated group of New York women (the Federation for Child Study) the exemplars for organized child study; second, he attempted to encourage women's colleges and women college graduates to incorporate child development into their definition of liberal education, and thereby to gain the prestige of their example for the movement as a whole; and third, he helped persuade LSRM to fund a popular magazine for parents to preempt the possibility of a more commercial venture. Each endeavor met with varying degrees of success, but each attested to the grand scale on which Frank had conceived the movement.[19]

This magazine later became *Parents Magazine*. Frank often spoke of "the climate of opinion," which he was not only aware of, but which he also believed could be manipulated to make it receptive to particular new ideas. Frank was the "raindancer" for the tribe of social scientists, who knew how to "create a favorable climate of opinion" to make certain ideas grow and thrive.

The advent of time-saving household technologies—canned and prepared foods, ready-made clothes, washing machines, etc.—was contributing to the foundations of a favorable "climate of opinion." Before the First World War middle-class mothers hired nursemaids to care for the young. But a systematic perusal of advertisements in popular magazines shows that after the First World War the current concept was that mother knows best how to raise her own babies and that relegating that particular function to others is irresponsible. Household technologies were advertised as "mother's helpers," whereas the premise in prewar advertisements had been that the middle-class mother relied on hired help. So it became a challenge and matter of pride to be a "good mother," and trustworthy information and guidance for mothering was in demand in the 1920s. The return to normal family life was another postwar theme favoring the study of child development. The circumstance favored a change in family pattern in the direction of increased focus on children, and the kind of research and education Larry Frank was fostering served to reinforce this popular interest.

In terms of Frank's own life the above is a poignant story of sweet success. The boy whose father had withheld financial largesse, who together with mother, brother, and grandmother suffered hardship and poverty and perhaps pains and obstacles in his own development, had a strong sense of the father's neglect. How delicious to be able to use the resources of paternalistic philanthropies in the service of parent education, child development, and the like, to promote the child-centered family, to make better ways of growing up, and to develop the knowledge of humane parenting for present and future generations, as well as to bring science and money to what traditionally had been women's domain. Frank was the father of seven children. He became a widower twice, but married thrice. Nor did his interest in child development and parent education abate: with his third wife, Mary, he wrote "how-to books" for parents: *How to Help your Child in School* and *Your Adolescent at Home and in School;* and in his last writings, when his own life had already spanned three-quarters of a century, he wrote *The Importance of Infancy.*[20] In this last book he wanted "to focus attention on infancy as a complex series of interrelated events, involving different disciplines and professions, agencies and individuals, with far-reaching implications for the future."[21] He still viewed the application of the latest ideas from science to infancy as pivotal to social progress. He seemed to have no qualms that his approach might entail an intrusion of the public into the private domain or might constitute subtle manipulation and control. When I interviewed the frail seventy-eight-year-old Frank, then suffering from heart ailments, he was in his living room surrounded by opened books and journals on diverse subjects. He was living in a modest home in Belmont, a Boston suburb. A teenaged son was wandering about looking for a book by Edgar Friedenberg. Mary was about to leave for New York, where her father had just died, and I recall Frank's solicitous advice that she sit on the east side of the train to avoid the afternoon sun.

If one looks at Frank's writings over the years, it is apparent that he not only believed in the importance of infancy but again and again looked to the latest ideas from science and the accompanying "climate of opinion" as the hope for the future. The infant is, after all, the perfect symbol of the future and the focus of hope, and Frank's programs carried that appealing message of hope for the future even to hard-headed foundation executives and university administrators. A number of persistent themes and attitudes deriving from various branches of scholarship appear in his eclectic thinking and writing. In view of his later participation in the "teleological mechanisms" and cybernetics conferences, it is interesting that Frank's first response, in 1929, to Walter Cannon's idea of "homeostasis" was to write to him that he, Frank, had "gained the impression that the young child, and particularly the infant, has a rather indifferent capacity for homeostasis and that the course of the development of the child may be regarded as the achievement of the more or less steady state of maturity," in which homeostatic mechanisms function reliably.[22] Frank used that observation to provide an argument and partial scientific justification for creating the new field of "child development as distinct from the problems of the constituent sciences which must participate in the study. . . ." Cannon's reply confirmed Frank's impression by reporting that "the control of temperature, for example, is very poor in the infant, and is only gradually achieved." Cannon had already then (1929) suggested to Frank an extension of the idea of homeostasis, in some respects foreshadowing what became common currency for some of the cybernetics conferees in the 1950s, namely, that

> there are interesting analogies between homeostasis in the individual's internal environment and his relations to the outer world on the one hand, and the conditions in the home as an internal environment for the family and the outer social, economic, industrial and commercial world with which the individuals in the home must relate themselves and which may thereby introduce disturbing factors into the stable conditions of the family group. . .[23]

In keeping with the consensus of the tribal council, Frank's 1929 notions surfaced refurbished and elaborated in 1966 in the form of his suggestion of a "scientific" model of "the infant as a General Purpose System," in fact a "self-organizing, self-stabilizing, self-directing, largely self-repairing, open system."[24]

Mead's thinking about infants was also stimulated by cybernetics. In the 1940s a concept among psychiatrists was the "rejecting mother" or the "schizophrenogenic mother," but emphasis on circular causality at the cybernetics conference led Mead—in discussion with Bateson—to think in terms of the mother-baby interaction in which the infant might initiate and perpetuate a cycle that results in the mother's disengagement from the child.[25]

I have characterized Frank metaphorically as a raindancer for the social sciences and related fields in that he influenced the largesse of philanthropic foundations for specific research and in other more personal ways helped a field of study to grow and thrive—although he was not a researcher in the field. His personal characteristics were such that he was taken seriously by the boards of trustees of large foundations; at the same time, even when he adopted mechanical metaphors, he understood much of the sensibilities of women and babies. He was the bringer of new, stimulating, hopeful ideas, the latest in science to help human happiness. He was a salesman who brought "scientifically certified healing" for the sickness of "society" viewed "as a patient," to use his own idiom. His conversation was laced with fantasy and imagination for hopeful futures. Harvard's

Henry Murray, one member of Frank's psychology-social science network described him as

> the procreative Johnny Appleseed of the social sciences, a peripatetic horn of plenty, crammed to his lips with everything that's new, budding, possible and propitious, an enlightened, jolly human being who has gone from place to place, from symposium to symposium, radiating waves of atmospheric warmth, cheerfulness, and hope, as he spread the seed for novel, hybrid, research projects to be nurtured, implemented, and actualized by others.[26]

He was very much in the American grain.

In 1936 Frank moved from the Rockefeller General Education Board to the Josiah Macy Jr. Foundation to become its executive secretary and assistant to the president; in 1938 he became executive vice-president and executive secretary. Already a major figure from having established the child development field in the United States, Frank actively participated and fostered other large themes in the social sciences, in particular those indicated by the phrases "fusion of psychiatry and social science," "personality and culture," "mental hygiene," and "psychosomatic medicine," and later, the focus on teleological mechanisms, cybernetics and world mental health.

Larry Frank was animated by the belief that twentieth-century social science would be able to liberate us from old superstitions, that each new result, each new successful conceptual formulation and interpretation in the social sciences and in psychiatry, was contributing to free us from ignorance, errors, and misconceptions about ourselves. The truth shall set us free, for it is ignorance and superstition that have been the source of much of the misery and suffering of people hitherto. It was his, as well as Mead's and Bateson's, central assumption (supported by anthropological and clinical-psychological evidence) that human nature is not fixed but adaptive and changeable. Human nature, i.e., personality structure, is contingent on social patterns prevailing in a culture, such as particular styles of child rearing, and can be altered by changing these cultural patterns. The connection between individual personality and the culture, however, is circular and has the formal structure: A causes B and B causes A. If certain human personality traits or social patterns are seen as more desirable than others (e.g., an authoritarian personality and extreme competitiveness were typically viewed as culturally undesirable), then changes can be brought about by exerting influence at any point in the circular causal system. The tensions in both individuals and social systems cause each to seek an equilibrium, a harmony. The dynamic pursuit of the forever-elusive state of harmony entails the constant flow of feedback information to guide it. From this view of human nature and society, some of the theoretical concepts provided by cybernetics seemed suitable for modeling people and cultures.

Frank's faith in the liberating power and usefulness of social science research was *not* echoed by, among others, McCulloch, who pursued science for the fun of it and who was not attracted to the utilitarian Baconian vision. To McCulloch, this would only lead to efforts at managerial meddling in people's lives and social arrangements, attempts to exert control inhibiting human freedom or to create dependency on self-styled experts. Bateson was closer to McCulloch than he was to Frank and Mead on this point. Frank's pragmatic faith in the merits of social science (and psychiatric) expertise, although popular among the "experts" themselves, took for granted the right and ability of the "experts" to guide the populace for the latter's own good, a presumption McCulloch and others abhorred.[27]

Meanwhile philosophers in Europe had been moving in other directions—phenomenology, existentialism, hermeneutics, and structuralism. These modes of thought highlighted the full complexity and paradoxicalness of humans who are experiencing subjects, yet treat themselves as objects of study and devise representations of themselves in terms of concepts. Postwar attitudes within the human sciences in Europe differed greatly from those espoused by Frank; they were more critical and less optimistic. Perhaps the most interesting and trenchant critique did not appear until some time later, when Michel Foucault presented his "archaeology of the human sciences." Foucault made the language of discourse among workers in the human sciences the subject of his historical metatheoretical investigation and asked what an adequate approach would be for representing the person, one that does justice to various dual aspects: The human "as a fact among other facts to be studied empirically, and yet as the transcendental condition of the possibility of all knowledge; as surrounded by what he cannot get clear about (the unthought), and yet as a potentially lucid cogito, source of all intelligibility; and as the product of a long history whose beginning he can never reach and yet, paradoxically, as the source of that history."[28] From that comprehensive perspective Frank's optimism was based on a simplistic view that overemphasizes the empirical and scientific and thereby distorts what it is to be human. Foucault is led to conclude the very opposite from Frank, namely that the efforts to depict the human in terms of the human sciences are self-defeating and in process of becoming obsolete!

The young Mead had already sought advice from Frank in 1931, while she was preparing for a two-year stay in New Guinea.[29] Mead's biographer has described how she became acquainted with Frank's style.

> As methodically as Mead had once surveyed new classrooms and new bunches of playmates, she now assessed the cast of characters in American social science, to figure out who among them would be most worth meeting and how she could best get to know them. At a New York party in 1934 Mead, for the second time, met . . . Frank, who told her about a big plan he had. . . .[30]

The plan took shape in the form of a month-long conference in Hanover, New Hampshire, and resulted, in Mead's words, in a "multi-front operation. . . . Pulling together all that we knew about human development as we would want to teach it in the schools . . . our first attempt to formulate all we knew, using the whole range of human sciences to do it." At the conference itself "Mead was as happy as a child at summer camp assigned ideal tentmates and a perfect counselor. Interdisciplinary convocations, from this point on, would be one of the sustaining delights of her life."[31]

Larry Frank believed that social science and psychology were the wave of the future, and his summer home, Cloverly, in Holderness, New Hampshire, became a locus during the summer months for active discussion among psychologists and social scientists. Guests and neighbors with nearby houses included Margaret Mead, often Gregory Bateson while they were married, frequently her close friend and mentor Ruth Benedict, and sometimes social psychologists Erik and Joan Erikson; regularly the psychologists Gardiner and Lois Murphy, sociologists Robert and Helen Lynd; and on occasion the pediatrician Benjamin Spock, social psychologist Kurt Lewin, and Frank Fremont-Smith among many others. Filmer Northrop also vacationed nearby, as did Norbert Wiener, who occasionally dropped in. These New Hampshire summers, "a dozen different households linked by intellectual effort,"[32] had the character of a community of seminal "insiders," an informal elite talking to one another and significantly shaping and informing the direction of the social sciences in the United States. Lois Murphy justly said of Larry Frank that he "was one of the most seminal figures in the social sciences. . . . He would pull people off their islands of separate scientific disciplines and get them onto a new island where they could communicate with each other."[33] Arranging a conference or an invitation to Holderness often served that purpose. Although he wrote a number of books, Frank was unique among this group of workers in the social sciences in that as a foundation executive he was often in a position of "angel." He could do a great deal to arrange for positions and to create institutes for the research areas and people he was promoting. In those days private foundations were the source of outside funds for academic research in the social sciences, for the government did not become a major funding source until the Second World War. Of course, opinions differ, but in Mead's view Frank was no ordinary foundation executive; instead he "was one of the two or three men who used foundations the way the Lord meant them to be used."[34]

Mead's daughter has given a picture of Mead and Frank together: "They *sat* and they *talked,* as they did through a dozen subsequent summers, getting up at six and rocking on the long Cloverly porch for hours on end . . . (Margaret Mead) spending her days in conversation with Larry or typing in a small cabin by the lakeshore."[35] The pleasure in and importance of intellectual conversation for Frank and Mead, both great talkers, cannot be overestimated. Here is a description of Mead talking with a different partner: During the war she and Kurt Lewin were collaborating in the study of the use of small groups to influence Americans' food habits: "She and Kurt were fabulous together. Both could talk a mile or two a minute. When they sat down together, she would look worshipfully and silently at Kurt, and after a minute something he said would set her off, and he would start stuttering, 'but—but—but—.' She would talk at him, and someone else would try to slow it down and one of them would say, 'Let's go after this—.' They would just go at things and finally come out with some agreement."[36]

Most of the seventy participants and guests at the cybernetics conferences were and are little known to the general public, but years before the meetings Margaret Mead's name had already become a household word, and she has continued to be widely and popularly known throughout her life. Even after her death books, articles, and television documentaries have kept the public informed about who Margaret Mead was.[37] She was not only one of the leading anthropologists of her generation, and for a time president of the American Anthropological Association, but also the great popularizer of anthropology. She became its representative to the world at the United Nations, in the halls of Congress (where she was always ready to testify on social issues at Congressional hearings), at the American Association for the Advancement of Science (whose president she was for a year), at the World Federation for Mental Health (which she helped to organize and also presided over for a year), and among many other groups. She addressed the general population in a continuous stream of books, articles, and talks, a regular column in *Redbook* magazine, and by frequent appearances on television talk shows and innumerable visits to local clubs, colleges, and high schools. She represented, explained, promoted, and negotiated for her profession and was an always articulate advocate and defender of the work of those social scientists she, Frank, and their circle found congenial. We may designate her function in the tribal council appropriately as that of "talking chief."[38] As an anthropologist she was perennially concerned with conversation across the interface between cultures or subcultures and with the kind of leadership that makes cultural contacts mutually beneficial rather than destructive.[39]

In Mead's most famous fieldwork, which she popularized for the general reader, she showed that standards and ideals of male and female temperament and behavior differ enormously from one culture to the next.[40] With advice and inspiration from Franz Boas and Ruth Benedict, her mentors at Columbia University, she had intrepidly set out alone at age twenty-three for postdoctoral fieldwork in Samoa to study Samoan girls' adolescence. In subsequent field trips she had, together with New Zealander Reo Fortune, studied the Manus tribe on the Admiralty Islands, and then the Arapesh, the Mundugumor, and the Tschambuli tribes in New Guinea. She later studied the Balinese with Gregory Bateson. She did not go native in these foreign cultures to the extent some anthropologists do but was always the alien visitor seeking information. Her work, as that of other Boas students, was informed by a sense of double mission: to record and describe the existing primitive cultures before Western civilization had disrupted them; and to illuminate the problems in Western society by what is learned. Her field studies made concrete the theme of the interrelation of culture and personality. She used her studies to appraise the Western ideals of male and female and Western family patterns, and showed them to be the concomitant of our particular culture. Mead's and Frank's views of matriarchy, patriarchy, and feminism are subtle and complex, and the findings about the link between culture and personality are subject to the most varied application to our society. She herself was not a feminist, and simplistic interpretation of her work promulgated by some feminists disturbed her. Yet her own and Frank's approach to social change was a gentle one: not via political and economic forces, but by changes through education in styles of personal relations, child rearing, family and sexual patterns, and promoting mental health. However gentle, it contained a strong element of the managerial, the manipulative, and the controlling. In the conventional use of gender terms, it was a matriarchal rather than a patriarchal style of producing change. The anthropological observers' intrusion into other cultures, as well, was less aggressive than that of military conquerors, business entrepreneurs, or even religious missionaries, but was nevertheless destructive of cultural privacy and self-containment. According to Mead the salient characteristic of anthropology was that it is

> A field science, whose members work with fresh field materials, studying living speakers of living languages, excavating the earth where archeological remains are still in situ, observing the behavior of real mothers' brothers to real sisters' sons, taking down folklore from the lips of those who heard the tale from other men's lips, measuring the bodies and sampling the blood of men who live in their own lands—lands to which we have to travel in order to study the people. We still have no way to make an anthropologist except by sending him into the field. . . .[41]

The Boas students were not the only group to recognize the relation of personality to culture as an important topic of research. Frank had come to it independently in the 1920s through his varied work experiences and his eclecticism. The Chicago School of social science (John Dewey, George Herbert Mead, Charles Cooley, Robert Park, and Jane Addams and Julia Lathrop of Hull House), which linked pragmatic humanism with social theory and an interplay of theory, empiricism, and practice, was a different source of thought along these lines. Independently, Harry Stack Sullivan was developing a theory of psychiatry that put the "interpersonal event" at the center. He was in the 1920s acutely aware of the need for research in psychiatry and sought to have the field recognized as a proper social science by foundations dispensing research funds, and to be represented at the Social Science Research Council.[42] A confluence of these various streams occurred when Sullivan contacted Frank, then at the Rockefeller Memorial, to seek funds and find moral support for his assertion that psychiatry is a social science. Sullivan was also in touch with Edward Sapir, a professor of social anthropology at the University of Chicago and one-time Boas student.[43] With some support from the University of Chicago, and Frank's cooperation, Sullivan and Sapir were able in 1928 to convene a First Colloquium on Personality Investigation, to be followed by a second one the following year.[44] In 1932, with Frank's help, a seminar (taught jointly by Sapir and Dollard) on culture and personality was set up at Yale for Rockefeller Foundation Fellows. By the time of the cybernetics meetings the various streams had joined to become a river mighty enough so that the new department of social relations at Harvard would be formed around it. The personality and culture theme needs, however, to be delineated from the connections between human personality and the nature of society made by some thinkers who were refugees from Europe and had analyzed the rise of fascism there, such as Wilhelm Reich, who had argued that "character formation," and the resulting personality "depends upon the historical-economic situation in which it takes place."[45] Horkheimer and other members of the Frankfurt School had similarly emphasized the influence of political and economic conditions on personality formation.[46]

Granted that personality and culture could be viewed as a cybernetic system with purposes, feedbacks, and communication links, which if analyzed might lend credence to the notion that individual psychological change could be an effective route toward ameliorating the whole society. Still, Mead and Frank lived in a different intellectual universe from that inhabited by Pitts, McCulloch, or von Neumann. At the cybernetics meetings some interpenetration of these two universes took place.

The discussions at the meetings were given a form that outlived the discussants when the transactions of meetings six to ten were published. In editing the transcripts, assistant editors Mead and Teuber disagreed and fought out their differences; Mead, the senior of the two, on the whole got her way. Mead had been at the conferences from the first, whereas Teuber had first joined the group at the fourth meeting and von Förster only at the sixth. Von Förster, the editor, a recent immigrant from Austria, was primarily concerned with learning and using the English language and doing the editorial job well.[47] Mead, the only native American-English speaker of the three, had an anthropologist's interest in the group's words and actions. She would have liked best a film showing who sat next to whom, whisperings among those seated next to each other, facial expressions, and all sorts of nonverbal behavior, but having only a verbal record, she wanted to include the jokes, the asides, everything said, so as to make a comprehensive document for a detailed social study of a small intellectually substantial group at work.[48] Teuber preferred a record of only the scientific information and ideas presented, the substantive content, but not the "fluff." Many of the physiologists, mathematicians, and engineers would have sided with Teuber, whereas at least the core of the social science tribe, as well as Fremont-Smith, would probably have supported Mead. The conflict is one indication of how different the premises of participants can be in such a cross-disciplinary group.

The discussion at the tenth conference was particularly wild and diffuse and, in Teuber's view, was marked by a "relative lack of content." He recommended that the transactions for the tenth meeting not be published, but if others insisted on publishing he would resign as assistant editor![49] Mead sought to prevent his resignation, and after much five-way (Förster, Mead, Teuber, McCulloch, Fremont-Smith) discussion about the discussion, she proposed that the last volume consist, aside from an introduction by Fremont-Smith and a closing summary by McCulloch, of only the papers presented and that all discussion be omitted. Teuber did not withdraw. Mead then wrote to the presenters asking them to amplify and revise their papers for the volume, which finally appeared two years after the tenth conference.

A few words are in order concerning Heinz von Förster. He was Viennese, at home where art, literature, *Kultur* were appreciated. Before coming to the United States, he had been editor of the arts and sciences at the radio network set up by the U.S. Army Information Service in Vienna. Förster had acquired a doctorate in physics in the midst of war (1944) from the University of Breslau, Germany. Taking inspiration from Max Delbrück's theory that the structure of a gene, the unit of heredity, is "that of a huge molecule, capable only of discontinuous change, which consists in a rearrangement of the atoms,"[50] Förster hypothesized an analogous concept to describe memory. He supposed a unit of memory, the "mem," presumably a macromolecule whose energy is raised from the ground to a higher metastable quantum state when it is impregnated with a unit of information. Forgetting of a unit of information would correspond to the molecule's return to the ground state. To make his theory quantitative, von Förster fit it to the forgetting-curve for nonsense syllables that he found in a textbook on psychology. McCulloch had learned of the theory, which had been published in German,[51] and promptly invited von Förster to come to New York and attend the next cybernetics meeting. The theory was viewed with skepticism at the meeting, especially because the neurophysiologists could not find a way to link it to "any kind of picture we have of the nervous system."[52] Besides, by that time the Macy group had become imbued with a more plausible approach to memory based on viewing the brain as analogous to a computer.

In 1982 I had the opportunity to talk with von Förster, an urbane, gracious man who was bubbling with enthusiasm for cybernetics, in his house set on top of a steep hill in a wooded area close to the ocean about fifty miles south of San Francisco.[53] He recalled that he enjoyed the Macy conferences enormously but had been taken aback when McCulloch and Mead asked him to become the editor of the proceedings. As they realized, this task obliged him to become quickly proficient at written and spoken English. Today his spoken English is distinguished by its rapid rate and by the high frequency of words such as "splendid" and "delightful," expressing his general exuberance. With McCulloch's help he obtained a research position in electronics at the University of Illinois in 1949. From 1958 on, when he set up the Biological Computer Laboratory there, he devoted efforts (aside from administrative duties) to ideas that had grown out of the cybernetics conferences and to bringing other enthusiasts for cybernetics, such as Ross Ashby, as visitors to Urbana. He found a topic wherein cybernetics and physics overlapped when later he became intrigued with how to describe in mathematical language the circular interaction between an observer and an object observed.[54]

During the war von Förster had been employed on the German side as an engineer in microwave electronics, carrying out research on vacuum tubes to generate microwaves, a subject essential for radar, then at the forefront of wartime technology. He had been born into a wealthy family close to the Wittgensteins (Ludwig Wittgenstein was his mother's cousin) in Austria.

When I compared the unedited transcripts of the sixth and ninth conferences with the published, edited ones, I found that on the whole the published transactions are close to the unedited version. Some of the participants fleshed out or reworded comments that had been terse, incomplete, or confusing in the unedited version. Minor changes were made. When Fremont-Smith said "They don't know a damned thing about fatigue," it appeared in print as "We don't know much about fatigue."[55] When arguments had interrupted Kubie in his presentation about neurosis, a comment by Frank with a slightly satirical double meaning is omitted ("Can we hear the rest of Kubie's presentation? We want to get to the repetitive core of his and not ours.") First names Ralph, Frank, Larry in the discussion tend to become Dr. Gerard, Dr. Fremont-Smith, and Dr. Kubie in the published version. A rather compact presentation by Walter Pitts about synaptic transmission, which occurred at the end of the sixth conference, is omitted, presumably at Pitts's request. Some interchanges reflecting the sociopolitical context are omitted, although they are of considerable interest to the historian.

To get a sense of the quality of Mead's participation at the conferences, and to show something of the actual atmosphere, I will review the proceedings of one meeting. The first paper presented at the seventh meeting (23–24 March 1950) was by Ralph Gerard. It is devoted to amplifying and documenting the observation he had already made at the first meeting, that the brain functions more "analogically" than "digitally," and for that reason theories of the brain based on the Pitts-McCulloch model neuron may be inappropriate. For Mead that was merely a technical topic for neurobiologists, and while she presumably enjoyed observing and listening to the ensuing discussion, it had no relevance to her professional concerns and she did not enter into it. The rest of the papers at the seventh conference dealt with diverse aspects of language, a topic of great interest to Mead. All of the presenters, except Mead and Kubie, were guests rather than regular participants. J. C. R. Licklider was a relatively young (b. 1915) psychologist who after a year as research associate in Wolfgang Köhler's department in Swarthmore, had spent the war years at the Harvard Psychoacoustic Laboratory engaged in military research. He was one of the group of psychologists of his generation (such as Craik in England and, in a different way, John Stroud), for whom electronic engineering, psychology, and military requirements seemed to dovetail. His paper at the Macy meeting dealt with the ways and the extent to which speech can be distorted and yet remain intelligible. Pioneering studies on that topic had been made at the Bell Telephone Laboratory in the 1920s,[56] but the 1940s work at the Psychoacoustic Laboratory was technologically more sophisticated.[57] One military concern was the accurate understanding of a command by a subordinate, and the selection and training of people who can be relied upon to understand correctly.[58] Licklider described at the meeting how the sound made by a person speaking, as well as distortion of that speech and noise, can be analyzed mathematically. Three of the men at the meeting (Shannon, Bigelow, Wiener) had made major contributions to developing the theoretical tools for studying messages and noise generally, but to measure "intelligibility" Licklider went beyond that work. He and his coworkers asked human subjects what they could hear and understand from an oral message read to them. Licklider's measure of intelligibility was not unrelated to Shannon's famous measure for information; however, it was not only a function of the properties of the sound constituting the signal but depended as well on the characteristics of the human hearing mechanism. Among the psychologists present Teuber, Stroud, and Werner had experience testing people's hearing and comprehension, and a largely technical discussion followed Licklider's paper. Yet Wiener's advocacy is only thinly veiled as he repeatedly called attention to a particular nonmilitary application for techniques making verbal communications intelligible: He spoke of the "promising" developments in hearing-aids, the "fascinating" subject of prosthesis, and the presence of "much hope of working for the deaf."

Mead boldly raised the neglected topic of emotional tone of human speech not contained in the words themselves. "What order of distortion might be introduced to take the anger out of a message that otherwise will carry exactly the same words?" she asked. "Perhaps the most essential point would be sincerity. As we are going further into the continual broadcasting between countries, or use of broadcasting in various sorts of general communication, what does and does not sound sincere might be something that you could analyze. . . . The question is whether that would be manipulable."

Mead's social and political concern showed behind the technical question. The brief attention it received was cut off when Klüver, with some sarcasm, said that he "should like to bring up a point which has fortunately or unfortunately no bearing on the international situation," as if Mead's comment had been out of order in this "scientific discussion." He proceeded to talk about the speech of parrots. Later on, as the discussion wandered, Mead made the point that one person may see two languages as essentially alike whereas another person will regard them as fundamentally distinct, and consequently "what is translation for one person is not translation for another." Well, then, do machines provide an objective criterion for what constitutes "translation"? That question provoked lengthier discussion.

Licklider's study of intelligibility entailed mathematical analysis of sound as well as quantification of human characteristics. The succeeding paper, a report by communication engineer Claude Shannon, on a "recently developed method of estimating the amount of redundancy in printed English," neatly avoided all consideration of human characteristics, just as his famous definition of "information" had.[59] In Shannon's analysis of printed English the statistical frequency of letter combinations is fundamental, but the "meaning" of a message is taken to be irrelevant. Redundancy is of interest to the communication engineer primarily because, to achieve high efficiency, one would like to use codes that minimize it. Mead surfaced on three occasions in the ensuing conversation: In one comment, in defense of redundancy, she described examples showing that when people talk to each other, redundancy is nearly indispensible, making it possible for people to listen comfortably and to comprehend each other. When Teuber raises the possibility that the degree of redundancy of a language might be a good measure of its "primitiveness" (the more primitive the more redundant, presumably), Mead in her role as expert points out that Teuber's notion does not fit actual primitive languages, because some of them are not very redundant. The third place where Mead appears is a little side conversation, unconnected with the rest of the discussion, among Brosin, Frank, and herself about psychiatric diagnoses. On the whole Mead's comments, in connection with both Licklider's and Shannon's papers, pointed to basic human characteristics that tended to be forgotten in the mechanistic frame of these authors' analyses.

Mead's own paper was introduced by McCulloch who announced that she will give "a picture of how one learns languages if he does not know the languages of that family or the culture of the people, languages for which there is no dictionary. That is a situation in which an adult consciously attempts to break a code." Cracking a code is the kind of puzzle that piques the interest of mathematicians, computer engineers, and theoretical neurobiologists, and the notion of "code" linked Mead's topic to one of the persistent themes of the meetings. Mead's interest was distinct from that of a professional linguist: an anthropologist entering a society and, with no foreknowledge of it, wanting to learn to speak its unwritten language—a language with no structural relation to the Indo-European or any other familiar language group. Speaking from her own experience in New Guinea, Mead discussed the role of a *lingua franca,* especially pidgin, and the advantages of learning the new language from children rather than from adults (children are closer to the learning process themselves). She described the special importance of listening, "letting them talk," of learning to ask questions correctly, of giving commands accurately, learning "to give signs of grief, pleasure, and other emotions which are verbalized in the society," which she contrasted to the relative unimportance of declarative sentences. The discussion that followed dealt with many specific points, but it is evident Mead succeeded in communicating to mathematicians because the two most active discussants were Pitts and Wiener. Pitts's questions show that he was avidly interested in understanding the process of learning Mead described, but also that his deeper concern was with logical structure and universal elements in language. What are the logically necessary elements of a human language? His comments highlighted the difference between an artificial, but logically adequate, language and natural languages observed and recorded. Following one of the comments, Licklider offered an interpretation of the distinction: "It could happen logically but does not happen naturally, and the reason is . . . that all human beings have in common a brain that works in a particular way." Pitts, perhaps a bit cautious about adopting Licklider's conclusion, however congenial it might have been, acknowledged only that some fundamental ideas "are apparently common to all languages without being necessary in any sense." A distinction between the logical and the empirical approaches is reflected in the following interchange:

> *Bateson:* . . . for example, all people, as far as I know, have the notion of a person . . . that is probably used for a very great deal of differentiation of objects, the notion of actions, and so forth. You start from that level rather than from what is logically necessary in an abstract way.
>
> *Pitts:* It is not a question of what you start from but what is absolutely necessary. We are not concerned with what you start from in one sense of building out but with what we must have when we finish our construction.
>
> *Bateson:* Those are two different questions . . .
>
> *Pitts:* I am sure we should keep them separate.[60]

Clearly, different participants brought disparate agendas concerning language: Licklider's analysis of the sound, Shannon's concern with transfer of "information" without regard to meaning, Mead's pragmatic interest in acquiring speaking knowledge of exotic languages, Pitts's formal and necessitarian objectives, and Bateson's empirical-yet-theoretical reflections on process.

So far no one had spoken about language from a developmental point of view, but Heinz Werner's paper "On the Development of Word Meanings" followed Mead's. Werner, a developmental psychologist, had emigrated from Germany to the United States, and at one point in the thirties had contacted Frank to help him locate a position.[61] His paper was based on the conjecture that the development of word meanings in children has much in common with the pattern of verbalized thinking of schizophrenics, as it had been described by von Domarus. As mentioned earlier, McCulloch had not only helped von Domarus in his work but had extolled his formulation of the logic of schizophrenics' thought as a step in the direction of a genuinely scientific psychiatry. Werner managed to span the distance between Frank's interest in child rearing and McCulloch's in the logic of mind. Werner had devised test questions that he presented to children to illuminate his hypothesis, and he presented this material to the Macy group. Mead was evidently a comfortable participant in the discussion. She called attention to the fact that a child's interpretation of the test questions is influenced by the context within which the questions are being asked. Even the notion of what constitutes a sentence, she said, is different for English people and Americans, and more generally meaningful only "within a specific cultural context."

Next, Stroud, the proud father of a one-year-old daughter, described a hypothesis about how the world appears to children at different ages, but no time was allotted for discussion of his hypothesis. The final paper by Kubie was on "the relationship of symbolic function in language formation and in neurosis," a psychoanalytic view; it will be discussed in chapter six, which deals with psychiatry.

Anyone who took in fully all that was presented during those two days obtained a richly textured, polyglot view of a wide range of attempts to understand language scientifically. (The fifth meeting, with Roman Jakobson, Dorothy Lee, Charles Morris, and Eilhard von Domarus among the guests, was also devoted to language.) Within a decade linguistics received a powerful stimulus from Noam Chomsky's formal work, which proclaimed a deep, universal structure underlying language. But logicians and the more empirical social anthropologists continue today to have disparate objectives in studying language.

Mead thought and wrote a good deal about groups such as the cybernetics group, and more than either Fremont-Smith or Frank, she provided a cogent, albeit somewhat romantic, rationale for them.[62] She thought such groups extremely valuable in the context not of biological but of "cultural evolution." In a general way she thought about clusters of individuals, which included at least some exceptionally innovative and bright people, who jointly and consciously played a role in channeling social and cultural developments. While she objected to attempts to control biological evolution or to manipulate people by behaviorist psychologies, she thought such clusters recognizing present problems and crises are the best, most American in spirit, and most promising units generating the innovations required for cultural viability and progress:

> The unit of cultural micro-evolution is a cluster of interacting individuals who within the special conditions provided by period and culture make choices which set a direction—a channel—in which events tend to flow until other points of divergence are reached. . . . The most distinctive characteristic of an evolutionary cluster is the presence in it of at least one irreplaceable individual, someone with such special gifts of imagination and thought that without him the cluster would assume an entirely different character.[63]

At the cybernetics meetings Wiener was unquestionably such an irreplaceable individual, and the group was perceived by Mead as an important evolutionary cluster. As she saw it, "out of the deliberations of this (cybernetics) group came a whole series of fruitful developments of a very high order."[64] Her construct of "unit of microevolution" would also fit the cyberneticians, as well as the cluster we have labeled the "tribe of social scientists" with a locus in Holderness. We shall encounter others.

Focus on the small group as a unit was in deliberate contrast to both the traditional thinking in terms of a whole society (or nation) as a unit and the emphasis on the solitary genius as the creator of innovative change. Mead's great theoretical emphasis on small groups was entirely consistent with her own, Frank's, and Fremont-Smith's practice of active participation in many groups. Extolling the role of the small group as a unit is also of a piece with Mead's emphasis on the interpersonal.

Frank attended the Macy conferences on cybernetics regularly from 1946 to 1953, but he never gave a presentation. Of course his friends were glad to have his familiar face present. He came a bit more into his own when he set up a one-time conference on the same topic, under the auspices of the New York Academy of Sciences, to follow on the heels of the second meeting of the Macy group while participants were still in New York City. He chose Wiener, McCulloch, Hutchinson ("Circular Causal Systems in Ecology"), and W. K. Livingston ("The Vicious Circle in Causalgia") as speakers. A few excerpts from Frank's foreword to the conference show the nature of his interest in the topic as well as his style:

> The concept of teleological mechanisms . . . may be viewed as an attempt to escape from . . . older, mechanistic formulations that now appear inadequate, and to provide new and more fruitful conceptions and more effective methodologies for studying self-regulating processes, self-orienting systems and organisms, and self-directing personalities. . . . It is suggested that we look at this conference as an important, perhaps a major, step toward the new climate of opinion now emerging in scientific, philosophical, and even artistic activities. We are not only witnessing, but, by these meetings and discussions, actively participating in creating this new climate of opinion. . . . As I see it, we are engaged, today, in one of the major transitions or upheavals in the history of ideas. . . . When the social sciences accept the newer conceptions . . . and learn to think in terms of circular processes, they will probably make amazing advances. . . . Already, the fruitfulness of the newer approach has been shown in the *psychocultural approach* which has begun to illuminate the dual aspects of social-cultural regularities and of highly individualized personality activities.[65]

In *Nature and Human Nature,* published in 1951, Frank recapitulated his general views expressed in terms of a harmonious synthesis of ideas, although he sometimes resorted to vague, pleasant generalities. He gave his assessment of the characteristics of his own time and implicitly his perception of his own activities and those of his tribe:

> [historians] will probably agree on one characteristic of our present time: that it was (is) a revolutionary period, a time when in almost every field of activity and of ideas new patterns and new conceptions are being developed to challenge the old and, with sometimes extraordinary rapidity, to supersede the long accustomed ways of living, thinking and believing. . . . Today the whole world is entering upon a Renaissance as the traditional ideas and beliefs, the customary designs for living in each group of people, undergo progressive breakdown and dissolution.[66]

His perception is at first sight surprising. Looking back to the postwar era, we see it as a conservative period, with a prevailing return to emphasis on the traditional family and secure jobs in large corporations, an absence of political activism, and intimidation of academics by political anticommunist crusades. There was an ascendancy of clinical psychology and psychologism, some movement toward considerable success in molecular biology (which in its reductionism was, however, uncongenial to Frank), a progress in physics (quantum electrodynamics) largely rejecting more radical ideas in favor of carrying through with much ingenuity some of the calculations based on by-then conventional quantum theory,[67] and of course the notions associated with cybernetics. Nuclear weapons were also a recent development. What "revolution" did he have in mind? Frank wrote

> Today we can assert with full conviction that culture is a human creation, man's attempt to order and pattern his personal life and to provide for orderly group or social living. . . . This indicates that culture is not a superhuman system, final and unchanging, beyond man's reach and control; also it shows that we can and do change culture by modifying what we think and do and feel and what we teach and how we rear our children. Again, this new viewpoint, when once grasped, brings an immense relief and a feeling of freedom we have never had before under the older beliefs in a supernaturally imposed culture, sanctioned by immemorial tradition.[68]

He thus saw human freedom and power to control one's own fate and future conditions as the upshot of applying the new secular knowledge of anthropology, psychology, and circular causation. By individuals changing their ways, the culture is changed.

No political or intellectual revolution was in progress, but social change was. It is symbolized by the nature of the professionals' advice on how to care for babies and children. The book that dominated the field until 1946 was written by leading behaviorist psychologist John Watson, who saw the child as clay in the hands of the adult molding it into the desired person by use of conditioning principles, but always requiring deliberate control of the child's behavior by the parents.[69] When Benjamin Spock's book appeared in 1946 it became a best-seller (the best-selling book with a new title in the twentieth century!), and a generation was raised by its advice, which relieved parents of common anxieties and emphasized that the child is a person in his or her own right whose wishes deserve to be taken seriously.[70] Spock favored breast-feeding, responsiveness to babies' demands, a relaxed, loving home environment, and in medical areas gave practical, commonsense advice. The book gave parents the kind of humane, generous, yet scientific advice Frank had been championing for two decades. It is no accident that Spock was part of his circle, and that Mead's raising her infant daughter in the early forties had provided Spock an instance of the breast-feeding and "self-demand" he later recommended.[71]

The implications of the freedom to break with traditional gender stereotypes, and with cultural conventions in patterns of living, resulted in relatively long-term social changes that probably exceeded his anticipations. Survey researcher Daniel Yankelovich reported in 1981:

> What is extraordinary about the search for self-fulfillment in contemporary America is that it is not confined to a few bold spirits or a privileged class. Cross-section studies of Americans show unmistakably that the search for self-fulfillment is instead an outpouring of popular sentiment and experimentation, an authentic grass-roots phenomena. . . . It is as if tens of millions of people had decided simultaneously to conduct risky experiments in living, using the only materials that lay at hand—their own lives.[72]

He provides a statistical measure as an indication of change since the 1950s:

> In the nineteen-fifties a typical American family consisting of a working father, a stay-at-home mother and one or more children constituted 70 percent of all households. This was the norm, the familiar American nuclear family . . . this norm has collapsed in a single generation. Far from being the dominant mode, the *"typical American family"* does not now constitute even a large minority of households. Rather it accounts for only 15 percent of them. There are fewer "typical American families" today than households consisting of a single person. . . . We have moved to a more variegated society with many types of households, no one of which predominates.[73]

These are significant changes indeed in personal lives, in many respects liberating changes, notwithstanding the new tensions they created.

In sum, then, Frank and the mental hygiene and social science he championed appear within recent history as both cause and effect. Foucault is convincing when he points out that the heavy empirical bias of the human sciences leads to a simplistic and distorted representation of what it is to be human. Moreover, the collective phobic response to anything smacking of socialism, the collective tendency to ignore poverty, the collective optimistic faith in techniques and technologies (hiding perhaps a sense of helplessness in the face of the destructive power of the new nuclear weapons)—that has been called technocratic optimism—and the neglect of historical studies, all contributed to fundamental misconceptions about society. That is not especially remarkable, because every era, including the present one, is characterized by prominent misconceptions held by the intelligentsia, among others—misconceptions likely to be brought into relief only by the next generation.

The impulse guiding Frank and Mead in their social actions was a genuinely humane and generous one. Although Frank saw himself as overthrowing traditions, in fact he appears beholden to the traditional ideal of Christian love, informed by the latest in the human sciences. The notions from cybernetics, some of them invented by men far more fascinated with becoming do-it-yourself creators of minds than with the amelioration of society, were to Frank grist for the mill. The cultural changes Frank, Mead, and friends consciously helped to bring about, such as changes in patterns of child rearing, sex education, emphasis on "personal growth," and more realistic and flexible perceptions of gender, were indeed experienced by many people of a generation younger than Frank as liberating. Nothing can take this constructive, human impact of the kind of pragmatic effort in which Frank and Mead engaged away from them. It is part of their legacy.

But to look no further would be naïve. The movement also contributed to giving social scientists and psychiatrists the elite status of priests presuming to know how we should live. A still younger generation, especially feminists, have looked critically at the "scientific" advice of experts, however benign their intentions, and found it shot through with biases that changed from decade to decade. Thus Mead's views have come under attack specifically from Friedan,[74] and the child-rearing recommendations of Frank and Spock from Ehrenreich and English.[75] These critics' recognition that consensus among a tribe of social scientists reflects the values, interests, customs, and knowledge of the tribe implies that the tribe's purported wisdom may not be appropriate for you or me. That is an outcome Frank did not anticipate. But after all, how could they expect to be exempt from the very cultural relativity that Mead and her fellow anthropologists had insisted on for everyone else? Furthermore, the movement's emphasis on the personal and psychological served to deflect people's efforts away from the possibility of changing fundamental political and economic structures.

Gregory Bateson distanced himself from others of the tribe by not offering the kind of advice for living that Mead and

Frank put forth. He saw himself as only seeking knowledge and understanding. From the first he had been most strongly convinced that the ideas from Wiener, McCulloch, and their cohorts were worth attending to for social scientists, and he had encouraged the others in that belief. Whereas Frank was a steady resident either on Perry Street or at Cloverly, Bateson came and went. "When Gregory Bateson was part of the crowd at Cloverly—'ARRIVING . . . MIDDAY TOMORROW SATURDAY WITH MUSIC AND CHEESE,' he once telegraphed—everyone had fun."[76] In many respects his attitudes and outlook differed from Mead's and Frank's. By training, habit, and inclination he was a naturalist. Recalled his daughter, "He had a genius for finding creatures. In the woods he would see the loose panel of bark on the side of a dead tree and reach in and find a bat, with a ferocious tiny face, nestled to sleep for the day. I learned to hold them, testing the small tenacious claws, and keeping them from taking off into flight from the edge of my palm. We caught snakes, pouncing to grasp them at the neck."[77] In his intellectual forays he displayed a similar style and talent. But at the cybernetics conferences he was seeking powerful abstractions, to help identify the patterns and find some universality in the diversity displayed in what he liked to observe, and perhaps simultaneously to obtain some distance from the concrete, immediate, and empirical. This was especially true after the war, when he was far less inclined to pragmatic "meddling with society" than were Mead and Frank, and he even warned against the presumption of social scientists' knowledge. Bateson could recall the presumably noble cause of the First World War, "the war to end all wars," which had cost him the loss of one brother directly, and indirectly of a second brother. And in 1946 he had just emerged from two years of war work "in defense of democracy" with the Office of Strategic Services in Southeast Asia: application of his knowledge of psychology and anthropology to undermine the morale of the Japanese, an activity that in fact undermined his own morale and left him profoundly depressed.[78] Becoming acutely aware of the limits of the positive sciences, he sought to do justice to the mystic as well as the scientist:

> The mystic "sees the world in a grain of sand," and the world which he sees is either moral or aesthetic, or both. The Newtonian scientist sees a regularity in the behavior of falling bodies and claims to draw

> from the regularity no normative conclusions whatsoever. But his claim ceases to be consistent at the moment when he preaches that this is the right way to view the universe. To preach is possible only in terms of normative conclusions.[79]

No enthusiast for such ventures as the World Federation for Mental Health, Bateson preferred an attitude of scientific exploration in the social sciences. He closed a lecture in 1959 with the words,

> We social scientists would do well to hold back our eagerness to control that world which we so imperfectly understand. The fact of our imperfect understanding should not be allowed to feed our anxiety and increase the need to control. Rather, our studies could be inspired by a more ancient, but today less honored, motive: a curiosity about the world of which we are a part . . .[80]

Although a devoted and original researcher, Bateson held no regular position at any academic institution. His style and status in relation to particular professions in the human sciences was an idiosyncratic one, which one perceptive reviewer has characterized metaphorically as that of "scout":

> In the literature and movies of the American Frontier the scout is usually depicted as a roughly clad eccentric who leaves the safety of the settlement and reappears unpredictably, bringing a mixture of firsthand reports, rumors, and warnings about the wilderness ahead—together with a tantalizing collection of plant specimens, animal skins, and rock samples, not all of which are fool's gold. At first the settlers find the scout's help indispensible; but once their community begins to consolidate he becomes a figure of fun; and finally, after respectability has set in, he is a positive embarrassment. Yet their premature respectability is vulnerable. When the settlement is struck by drought, the scout's nature lore leads the settlers to hidden springs of underground water, but once the crisis is past, respectability reemerges, and the scout is ridden out to the town line. Within the world of the American behavioral sciences, Gregory Bateson has always had the scout's ambiguous status.[81]

Among the members of the tribe Bateson's "mixture of firsthand reports, rumors, and warnings about the wilderness ahead" and his "tantalizing collection of plant specimens, animal skins, and rock samples" were appreciated, and his thinking was regarded as an important resource. Bateson will be described more fully in the following chapter, not only as a "scout" but as a synthesizer of diverse ideas, relying heavily on those he first encountered at the cybernetics meetings, to define a personal world view.

The metaphors raindancer, talking chief, and scout serve to suggest the activities characteristic of Frank, Mead, and Bateson in relation to a large group of researchers in the human sciences in the United States. No more detailed sociological analysis will be attempted. Not only do individual social scientists have unique styles of their own, but for the professional pursuit of the human sciences to thrive, it is clearly helpful—*indispensable* is too strong—if subgroups of the collectivity include people functioning in ways akin to a raindancer, a talking chief, and a scout, and if within the group a community is generated where mutual appreciation of talents, strong intellectual interactions, and cooperation as well as personal friendships are the rule.

However influential Bateson, Mead, and Frank were in determining which social scientists would attend the cybernetics meetings, the final decision was McCulloch's. In at least one instance, that of Erik Erikson, the tribe of social scientists could not make one of their own into a regular member of the cybernetics group. The scope of the interdisciplinary group was broad, but it did have limits, and excluding Erikson's area and style of research helped to define the boundaries of the group's inquiry. He came once, but the cyberneticians opposed reinviting Erikson a second time and had their way; Erikson in turn had had misgivings about the group.

Like Kubie, Erikson was a psychoanalyst, but he was neither medically trained nor much interested in neurophysiology—nor even in proving that he was "scientific." He had been part of the inner circle close to Sigmund Freud's family in Vienna, where he had gone as a young man in the role of an artist and teacher of children, and where he was psychoanalyzed by Anna Freud. His writing was not characterized by scientific, logical reasoning but by a literary, artistic sensibility, painting pictures of people and human situations with words. His sensitivity to individual life histories included awareness of social, historical, and cultural contexts. His style of thought and language were not that of the cybernetics conferences.

Erikson had come to the United States in 1933, where he soon met Margaret Mead, Gregory Bateson, Lawrence Frank, and Kurt Lewin, "people whose ideas helped shaped the direction of his entire professional life."[82] Without repudiating his identity as a psychoanalyst of children, he became part of the inner circle of the elite tribe linked to Mead, Bateson, and Frank, with its geographic center in Holderness, New Hampshire. He also came to learn about and participate in social anthropological studies, with an emphasis on children, of the Sioux Indians in North Dakota and the fishing culture of the Yurok tribe on the Pacific coast. He examined other cultural patterns by studying Nazi Germany (specifically Hitler's childhood), Russian characteristics (specifically Maxim Gorky's youth), and the American identity. At the time he attended a cybernetics meeting he was on the faculty of the University of California, intensely engaged in working with all these data, using them to extend and reformulate Freud's ideas, and preparing a book presenting the material together with his new viewpoint.[83] In the foreword to this book, which appeared in 1950, he acknowledged that "over the years in this country I had the privilege of long talks and short field-trips with anthropologists, primarily Gregory Bateson, . . . Margaret Mead, . . . It would be impossible to itemize my over-all indebtedness to Margaret Mead. My comparative views on childhood developed through research to which I was first encouraged by Lawrence K. Frank. A grant from the Josiah Macy Jr. Foundation enabled me to . . ." and so forth. He too had a special function in the tribe whose raindancer, scout, and talking chief we have already identified: He had, so to speak, grown up and come from another tribe—the German-speaking Central European Freud circle—where he had been a young favorite, before he migrated and was invited to the Holderness elite community. He became adept at the ways of his new tribe, but adapted them and psychoanalytic tradition to each other, thus creating his own new synthesis.

To prepare him for participation in the cybernetics meeting, to which he had been invited just after Lewin died, Mead wrote to him:

> Wiener initiated a discussion of the way in which the conceptions current in the different languages which had dealt in logic were discussed so as to bring out the fact that they could be thought of as "read in" not "built in" to the human nervous system. This distinction between the inherent form of the nervous system and the various formulations of experience which are reduced to form, in the course of learning within human culture, and thus "read in" seems to be a crucial one.

> And here your chart comes in. . . . Larry and Mary (Frank) are made very happy that you are coming, as are we. Ruth (Benedict) will want a chance to talk with you while you are here. . . . Gregory will hope to have a good day with you to bring you up-to-date on the material. . . . We are delighted that you are coming, delighted from half a hundred points of view. . . .[84]

From McCulloch's summary of the third conference, we get an idea of Erikson's presentation: "The rest of the (March 13, 1947) evening was given over to Erikson's presentation of child psychiatry in which he employs his famous diagrams indicating the normal path together with its pathological deviants from infancy through adolescence. This scheme centered around the relative importance of sundry orifices and their functions and its importance at stage after stage of development, going from mouth to anus and thence to genitalia."

Within a few weeks after the conference both Bateson and Hutchinson urged that Erikson be made a regular member of the group, although Bateson acknowledged that Pitts appeared to be opposed to the idea and added, "I doubt whether any of us social scientists will come up to his standard for another thirty years."[85] That was the objection: Erikson's lack of rigor and logical reasoning, his lack of precision, were anathema. His verbal approach, "painting of pictures," did not fit in with the cyberneticians' aspirations to "scientific" rigor. Erikson commented years later that, on his side, he never particularly warmed up to the subject matter of cybernetics.[86] Bateson told me, not without laughter, that he remembered Erikson's "coming out of the meeting horrified of this talk of machines, commenting that everyone around the room had a tic. He attributed this to the viewing the human being as a machine."[87]

Had Erikson become a regular member of the group, his approach to psychoanalysis would have been treated in the chapter on "deranged minds, artists, and psychiatrists." Within the spectrum of the practices and viewpoints of psychoanalysis, his community—Bateson, Mead, Frank—represented nearly the opposite pole from Kubie's, which was dominated by the medical profession and the rigidity and narrowness of interpretation prevailing at the New York Psychoanalytic Institute.

The controversy about inviting Erikson is one of many at the meetings. The following chapters deal with some substantive controversies and sharp differences among the conferees.

5

Logic Clarifying and Logic Obscuring

Now might I do it pat, now he is praying;
And now I'll do it. And so he goes to heaven.
And so am I revenged. That would be scanned.
A villain kills my father; and for that,
I, his sole son, do this same villain send
To heaven.
Why, this is hire and salary, not revenge!

Hamlet, act III, scene 3

Shakespeare's Prince of Denmark had choices. He considered the likely consequences of his conduct, such as the possible action of killing his uncle while he is praying. He also conscientiously gained further information by experiment: "I'll have these players/ Play something like the murder of my father/ Before mine uncle. I'll observe his looks,/ I'll tent him to the quick; if he but blench,/ I know my course . . ." The modern decision theorists would have approved both those aspects of Hamlet's decision process. But they might be a bit uncomfortable with his nonutilitarian criteria for choice, such as "whether 'tis nobler in the mind," and they might uneasily wonder whether his comment, that "There are more things in heaven and earth, Horatio,/ Then are dreamt of in your philosophy," applies to them as well. Hamlet's concern lest "the native hue of resolution/ Is sicklied o'er with the pale cast of thought," and whether "thinking too precisely on the event" is an excuse for cowardice are outside of the mathematical decision theorists' domain of discourse.

It was also in the sixteenth century, the time of Shakespeare, that scholars in Italy began to go beyond the study of Greek and Arabic mathematics. Hieronimo Cardano (1501–1576) in particular analyzed games of chance mathematically, in effect founding the theory of probability, and thus providing what is probably the first mathematical procedure for smart decisions when gambling. Galileo Galilei (1564–1642), famous for other work, further supplemented Cardano's analysis for dice games.[1] These precursors of modern mathematical decision theory focused on decisions of a special kind—when to bet in simple games of chance—far removed from the kinds of complex political and interpersonal choices confronting Hamlet.

Modern mathematical (i.e., statistical) decision theorists, on the other hand, have extended the ideas far beyond those beginnings. They claim for their techniques a wide scope, including even complicated interpersonal and political choices. They claim a comprehensiveness for their procedures that includes all decisions by individuals entailing uncertainty about the consequences. Part of the procedure can be carried out by a suitably programmed computer. Enthusiasts for statistical decision theory, by calling attention to their technology, tend to obscure the fact that it in no way replaces the truly difficult and often decisive aspects in people making choices: How among possible ways to best understand and conceptualize themselves, the world about them, and the impact of their actions on both; how to take into account the seemingly irrational elements when humor, paradox, or a multiplicity of objectives come into play; when to trust impulse to action or habitual passivity and when to delay so as to collect data and calculate, and so on.

Leonard Jimmy Savage, a young mathematician, did not belong to the cluster I have called the cyberneticians. During the years he was attending the conferences he worked on the reformulation of mathematical statistics and developing a systematic procedure for making "wise" decisions. He is a particularly interesting figure among the originators of modern statistical decision theories because he took subjective judgments seriously and paid considerable attention to philosophical critiques of his own method. Gregory Bateson, on the other hand, had come to consider questions of wisdom via biology, cultural anthropology, observation of natural language, and some reading in the Russell-Whitehead *Principia Mathematica*. Aside from having come to these questions through disparate intellectual routes, Bateson and Savage had entirely different personal and social backgrounds as well. They present a study in contrasts. Although not professional philosophers, neither shied away from reflecting about pertinent philosophical issues. Good decisions concern us all.

The interdisciplinary conferences presented a setting well suited for manifesting their difference in style and outlook. For instance, the persistent theme of comparing computers and brains had led to characterizing mechanisms in the brain as either "analogical" or "digital," in spite of some ambiguity in the definition of these concepts. After Ralph Gerard had at the seventh conference spoken about the respective roles of the two types of mechanism in the central nervous system, Bateson characteristically interjected, "I am a little disoriented by the opposition between analogical and digital." While the discussion focused primarily on detailed neurophysiological data, Bateson kept insisting on a clarification of the concepts. "It would be a good thing to tidy up our vocabulary," he said. Bateson recalled the historic conflict in Great Britain between the geneticists, especially his father, and the statisticians, especially Karl Pearson, concerning whether variation of biological species formed a continuum or is discontinuous.[2] He also recalled that an analogous argument had taken place at the meeting which Köhler attended (see chapter 10): the continuum of the Gestalt theory vs. the atomism of the Pitts-McCulloch model. But in the discussion following Gerard's paper at the seventh meeting one detects a tug of war between those intrigued with clarifying the concepts and those interested in data and description of experiment. Bateson and Savage were tugging in opposite directions. Walter Pitts finally cleared the air as to the concepts by succinctly pointing out that "digital and analogical sorts of devices have been defined quite independently and are not logical opposites." Although Bateson had pioneered the use of films and photographs in the human sciences, words and concepts continued to be for him the essential mode for representing reality in the social sciences. It was especially because the concepts of cybernetics had arisen in connection with engineering and mathematics that Bateson looked to them as tools for overcoming the vagueness and ambiguity of the conventional language of social science. Thus careful and precise use of language was crucial.

During the discussion Savage (as well as Bigelow, Licklider, and others) apparently became bored and impatient. As to the "battle" concerning analogical and digital, Savage interjected, "The battle is whether the distinction is worth making or not."

He further demeaned the "concepts" by referring to them as mere "nomenclature." The authentic scientific description was for Savage always operational, mathematical and mechanical, never merely verbal. His outlook was close to the logical positivists. In his own work the role of concepts and words was to describe actual or contemplated operations that are used to define mathematical symbols, but the words or concepts had no particular importance in themselves.[3] He wanted to get on with a description of the technical details.

A similar tug of war reinforcing earlier alignments occurred at the ninth conference following a presentation of Gerard's dealing with the details of excitation and inhibition of neurons, with Bateson attempting to shift the discussion so that it was relevant to social scientists. He interposed, "I have been racking my brains to consider what order of question I can put which will, at least implicitly, focus discussion upon general philosophical and epistemological problems, even though it be verbally a domestic neurophysiological question . . ." and indeed he then produces two such questions.[4] He was bored by the "domestic neurophysiological questions" themselves.

Bateson would also have been bored by the details of the still-infant phage work of Max Delbrück, whereas Savage would have been fascinated. Delbrück was one of those guests at the Macy who, like Erikson, did not return. In Delbrück's case, however, cyberneticians Pitts, von Neumann, McCulloch, and others wanted him to become a regular member, but Delbrück was not interested. I digress to recount the diversity of approaches to genetics at the meetings, analogous to the diverse approaches to making wise choices, and its consequences.

Gregory Bateson had imbibed discussion of issues in genetics practically with his mother's milk. Having learned of de Vries's rediscovery of Gregor Mendel's work in 1900, William Bateson became the chief publicist for it in England, and when his third son was born, in 1904, he named him Gregory in honor of Mendel. William Bateson coined the word "genetics" to encompass the study of heredity and variation of species, and—although some of his ideas were off the mark—as early as 1907 he had anticipated that biochemical factors play a role in linking genotype to somatic changes.[5] These issues, which Gregory had been exposed to and become involved with in his youth, continued to tug at Gregory, and he became actively engaged with them again some time after the cybernetics conferences.

Gregory Bateson's interest in evolutionary theory lay in exploring the dynamic pattern of relationships among genotype, phenotype, and the environment generally, and in concrete instances, so as to properly understand the phenomena of adaptation and phylogeny.[6]

John von Neumann had an entirely different kind of interest in genetics. He knew Max Delbrück and his work on bacteriophage—primitive self-reproducing organisms conveniently accessible to experimental study—and as early as 1946 thought it would be a challenge to devise a rigorous description of them.[7] Within a few years von Neumann developed a theory of self-reproducing automata, although, with modern molecular biology only in its infancy, he did not explicitly apply it to genetics. At the third meeting (March 1947) von Neumann said that a geneticist should be invited to become a regular participant.[8] Delbrück would be particularly congenial to him.

It was decided that a geneticist should be invited: Aside from Delbrück, Spiegelmann, Sewell Wright, and Dobzansky were also considered.[9] Some thought in terms of a biochemist rather than a geneticist.[10] Bateson, as well as Hutchinson, favored Spiegelmann, although he was more a biochemist, and there was a question whether he could be regarded as a proper geneticist who would satisfy von Neumann.[11] Hutchinson wrote to McCulloch:

> My chief reason for wanting Spiegelmann is that I know he has a very great amount of significant material ready along the general lines of the conference, relating to enzyme systems and cell metabolism. . . . It would be extremely instructive for the group to hear about it. . . .[12]

Bateson and Hutchinson were presumably anticipating an application of cybernetics along the lines described by Quastler at the ninth meeting, which came concretely into its own in what Jacques Monod much later called "microscopic cybernetics."[13]

Max Delbrück, von Neumann's choice, was invited to the fifth meeting and was expected to become a regular member of the group.[14] But the first day of the fifth meeting, orchestrated by Bateson and Mead, was given over to the subject of language (Roman Jakobson, Charles Morris, Dorothy Lee were among the invited guests). The second day was dominated by Wiener speaking about the evolution of order from chaos and giving an analysis of mechanisms serving as Maxwell demons, and by Pitts making a formal analogy from molecular collisions to describe the establishment of a pecking order among chickens, a problem in mechanism for social organization. Both analyses assumed initial randomness. The final topic was the use of the concept "I," a topic with which Dorothy Lee had been concerned in language, and questions such as whether the word "I" originates only in a social context. But where did all this connect to Delbrück's concerns?

Max Delbrück, a physicist strongly influenced by Niels Bohr's thinking about "complementarity," had in the 1930s turned to biology with ideas about the physical basis of heredity. By the time he came to the cybernetics conferences, he was deeply enmeshed in experimental work on bacteriophage, optimistic that he and his coworkers were on the right track to solve "the riddle of life." Others were similarly fascinated and optimistic, and after the war Delbrück had brought together a sharply focused research and learning summer community, the "phage group." He "succeeded in creating at Cold Spring Harbor (on Long Island) that spirit of ceaseless questioning, dialogue, and open-armed embrace of a life in science which he had learned from Bohr—but with a down-to-earth American character and a good measure of his own high-minded intolerance of shoddy thinking."[15] The narrow focus on the one specific organism, bacteriophage, was an effort to reduce heredity to specific molecular mechanisms and to identify these and the structure of the molecules involved. Thus heredity would then be reduced to physics.

By contrast, the interdisciplinary cybernetics group was diffuse in its focus, and Delbrück had no interest in it. He was famous for his bluntness. Commenting on the fifth meeting, he later said: "It was vacuous in the extreme and positively inane. Genetics did not, and at that time could not, enter into it at all. Also I was not then, and have not later, been much interested in the areas of information processing in the vertebrate CNS, or in simpler nervous systems, and in computer analogues."[16]

It is noteworthy that the molecular biology pursued by Delbrück and the phage group was on a very different track from that of the cyberneticians. It attracted many physicists who had read Schrödinger's *What is Life?,* while cybernetics did not—with the exception of Förster, who was in any case more an engineer than a physicist. Physicists in the 1940s tended to believe in a "building block" model of the universe, in which the smaller the unit, the more fundamental the explanation. The conceptions of Rosenblueth, Wiener, and Bigelow related engineering and high technology to the behavior of organisms on a human scale, but they were not particularly to the taste of physicists because they did not reduce description to laws of physics or to molecular building blocks; in that sense were not reductionist enough. Nor did they make contact with quantum theory. The notion of information, however, purported to extend one respected principle of physics, the Second Law of Thermodynamics, and soon caught the attention of and was used by physicists.[17] The McCulloch-Pitts model as well was not an application of principles familiar to physicists, but rather those of symbolic logic. The topic was not amenable to the normal theoretical tools of physicists at that time. The problem of the mechanism of gene multiplication and heredity was a different matter. Indeed Schrödinger's book (1944), citing Delbrück's work, had proclaimed the possibility that the mechanism for heredity is a fundamental topic in biology that can be reduced to molecules and principles of physics. Not surprisingly, many physicists were attracted to it after the war, the more so if they were sick of science linked to instruments of death.

But Delbrück came and went, and the cybernetics group never had a geneticist or a biochemist as a regular member. Molecular biologists were relatively slow in adapting the ideas of cybernetics to their needs. And Bateson never changed his view that the ideas of cybernetics are of far greater fundamental import than molecular biology.

In spite of his impatience with "domestic neurophysiological questions" Bateson regarded the cybernetics conferences as an intellectual event of the first magnitude, very exciting, and in many of his later writings he generously acknowledged their enormous influence on his own work.[18] But in sharp contrast to Bateson, Savage saw them primarily as pleasant "bull sessions with a very elite group . . . ; I could never take seriously the ostensible purpose of coming there to blend our disciplines or to grapple with each other's problems."[19]

As a further illustration of Savage's and Bateson's different styles, we compare their responses to the important ideas of "information" in communication theory, and the relation of information to entropy, both of which had already been presented at the March 1946 meeting of the group. At that time the theory was new, and the classic articles of Claude Shannon and Norbert Wiener appeared in print only in 1948. How did Savage and Bateson respond to the new information theory?

Savage studied the Shannon paper, followed the theorems and proofs. He enjoyed Shannon's work, saw it as posing and neatly solving some consequential intellectual puzzles of a mathematical kind. Wiener had emphasized in particular the concept of "information," or "information-theoretic entropy," which had been introduced by him and independently by Shannon. Savage was not interested in putting this concept on a pedestal; it was solutions to problems that mattered to him, not the idea of "information." He did not even take to the idea, as others would, of using the concept of "information" to obtain the probability distribution when limited data is available.[20] Bateson, whose mathematical training was quite limited, had the opposite reaction. He was stimulated by Wiener's suggestion that in a broad historical context the concepts of information and communication theory were ideas whose time had come, not only applicable to engineering but of significant heuristic value for the biological and social sciences. Bateson noticed that psychiatric thinking had increasingly come to emphasize interpersonal communication, and he set out to relate the new emphasis in engineering to the shifting emphasis in psychiatry.[21] In fact, a qualitative analog to the communication-theoretic concept of "message" came to play a central role in nearly all of Bateson's subsequent work on human interactions and animal behavior.

Much of Bateson's pleasure in science arose from creating a connected understanding of the most diverse kinds of knowledge and experience of the world. He had quite naturally wandered in his own career from zoology to social anthropology, and from there to a description of psychiatry; nor would he stop there. He has said, "I grew up in a world that has the idea that some ideas are non-trivial."[22] "Information" is that kind of an idea. To Bateson, the elucidation of general principles that cut across scientific disciplines is a crucial part of what makes science interesting and gives insight into the nature of things. Savage, in contrast, was ill at ease with such general principles; he limited his scope, as he relied on the solidity of mathematical proofs, axioms, or mechanical processes. Savage's rejection of those general ideas so fascinating to Bateson expresses perhaps an innate modesty. Like many American scientists, he tended to be repelled by some far-reaching ideas, largely because of their grandness.[23]

More immediately pertinent to decision theory than "information" is the question of the existence or nonexistence of a hierarchy of values for a single person. It was well known that if several voters are to choose among three candidates A, B, and C, the paradox of the cyclical majority may arise: Even if each voter can list the candidates in his or her order of preference, it can easily turn out after counting the votes that the majority prefers A over B, B over C, and C over A—in other words, a circular preference.[24] But Warren McCulloch had asserted that in experimental aesthetics he had found instances in which preferences are circular even for a single individual, and that experimental psychologists observing animal behavior had found analogous results for rats.[25] Bateson was immediately intrigued. McCulloch had concluded that "for values there can be no common scale," according to which all can be arranged in hierarchical order. "Therefore, as to the good in the biological sense, and the beautiful in the sense in which it can be inferred from mere human preference, we indeed live in a world of many incomparable values, where fate compels us to choose whenever we act. . . ."[26] McCulloch was not defending irrationality, but he constructed a hypothetical pattern of neurons in the brain that could give rise to such nonhierarchical schemes of preference.

Bateson played with this idea and contemplated various possibilities for understanding the empirical result. For example, one might analyze a net preference as in effect due to several entities (subsystems) within one person, each maximizing a particular variable, and that could, like several voters, easily lead to a circular preference.[27] He also sought from McCulloch more information about the empirical data, but didn't get much. For Bateson, careful observation of human behavior was fundamental to the human sciences. Savage's attitude, when constructing his statistical decision theory, was entirely different. His theory required that a decision maker can construct a hierarchical list of preferences. He said logical consistency required it. The wise decision maker, according to Savage, will be willing in every case to construct a "logically consistent" hierarchical list of preferences for the various possible outcomes of an action. Thus, as he admits, his theory is intended to be normative rather than empirical.[28]

Born in 1917, Savage spent his boyhood in Detroit. His father, a Jew without much formal education who had changed his name from Ogashevits to Savage, was in commercial real estate. Jimmy Savage has written, "My grandparents were hopeful immigrants from a cruel country, and the elan of the great American melting-pot epoch came down to me in many ways through my father and mother, giving me some disposition to reach for the stars."[29] But the obstacle to the aspiring boy scientist was that his teachers seconded the result of his IQ test, which put him into the class of morons, and strongly recommended against his going to college. He suffered from myopia and severe nystagmus, eye conditions that doubtless contributed to his failure in high school.[30] But once in college his abilities were appraised differently, and he obtained his doctorate in mathematics from the University of Michigan at the age of twenty-three.

As a graduate student Savage had encountered von Neumann's classic 1928 paper on game theory, was intrigued by it, and appreciated its potential applicability to economics. His first published paper was in collaboration with an economist.[31] Savage spent a postdoctoral year at the Institute for Advanced Study, in Princeton, where he became further acquainted with von Neumann and his work. At the end of the year von Neumann offered him a position at the institute as his assistant. Despite his admiration for that extraordinary mathematician Savage turned it down, just as he did a 1946 offer to join von Neumann's computer group.[32] During the Second World War he joined the Statistical Research Group at Columbia University, where among his colleagues were the pioneer of decision theory Abraham Wald, the economist Milton Friedman (with whom Savage later collaborated), and statistician W. Allen Wallis. Savage was working in applied mathematics, and he came to think of mathematical applications to biology as his own line of work. After the war he continued consulting with biologists about application of mathematics, especially statistics, to their various research problems, and obtained a special Rockefeller Fellowship to pursue this at Woods Hole Marine Biology Laboratory and at the University of Chicago. After some years at Chicago Savage shifted direction. As he wrote in a fellowship application, he had found that "the work in biology, while constructive has tended to be intellectually rather elementary, so that, though I expect to continue activity in biology, most of my research for the next several years is likely to be in the more academic aspects of statistics and the closely related field of probability."[33] Just as Norbert Wiener's enthusiasm for linking mathematical and biological topics was riding high, Savage's focus had shifted away from biological applications to the foundations of statistics.

His idea was first of all to follow von Neumann and Morgenstern,[34] notwithstanding McCulloch's arguments, and introduce a person's "utility function"—a linearly ordered quantitative measure for the "value" one places on the various possible outcomes of an action. To construct a "behavioral statistics" or "statistical decision theory" also requires a clear definition of the "probability" for each possible outcome. Only then could one compute the expected utility of an action and pick out the preferred one. Here Savage parted company with Abraham Wald, R. A. Fischer, and the leading statisticians of his day, members of the "frequentist" school, who had defined probability in terms of the results of an actual or hypothetical experiment repeated many times.[35] Savage preferred to define probability not as the result of any objective experiment but as a measure of the strength of a person's belief in a particular proposition, as, for example, the bets he would in principle be willing to make that a particular event would occur. The "frequentist," or objectivist, school could not readily assign a number to the likelihood of a unique event, such as the probability that a particular person will be the Democratic nominee in the next U.S. presidential election, because it is a question not suited for repeated experiment.[36] On the other hand a person could give his "personal probability," as Savage liked to call it, about such a one-shot event. Before Savage had come to it, the personal probability concept had been used by Frank Ramsey in England and Bruno de Finetti in Italy, who had ideas similar to Savage's, although the underlying idea of a subjective probability goes back further to Pierre Simon de Laplace (1749–1827) and Jacob Bernoulli (1654–1705).

The task Savage set for himself was in terms of what he saw as the task of statistics, namely, "the formulation of satisfactory rules for action in the face of uncertainty." I. J. Good in England was independently working along similar lines. At the time of the conferences Savage was occupied with the exercise in logic and mathematics posed by the idea of reformulating decision theory or "behaviorist statistics" in terms of the two basic notions of utility and personal probability, an exercise in manipulating axioms, definitions, and symbols. As he was trying out axioms and definitions, the rules of the game were that the basic concepts (utility, probability) be related to actual possible operations in the spirit of Bridgeman's operationalism, and that the definitions and postulates were to be complete, consistent, and logically correct. In addition, he appears to have chosen concepts and postulates in a way that he found aesthetically and mathematically pleasing. His book *The Foundations of Statistics,* published just a year after the last of the ten Macy conferences, sums up his main original work of those years.

I met Savage in 1968 at Yale University, where I was a visiting scholar, and interviewed him twice.[37] He was a tall, moderately slim man with a gray beard and sideburns, extremely thick glasses, dressed in the usual professorial attire of slacks and sport coat. A slight nasal quality and expressive rhythmic changes in inflection in his voice characterized his articulate speech. He was a technological optimist who could expound eloquently on the wonders of penicillin, radar, transistors, and space travel, retaining some of the ebullience of the boy-scientist. Lest he take himself too seriously, he also collected spoofs on science: "It is good to laugh at ourselves sometimes."

His office was a large department chairman's office: the rugs on the floor were Navajo, the paintings on the wall were good reproductions of Breughels and an early Italian or two. Watching Savage pick up a letter and place it practically right against his face to read it, I was reminded of his eye condition and wondered whether he at all saw or could appreciate the paintings. When I later inquired about this, he responded, "I do see them and enjoy them in my way. The Breughels with all their interesting detail are a little more frustrating for me than the others." We sat down, he on a couch and I on a comfortable chair, and began to talk. While we were talking, Savage sitting on the couch with his legs crossed, he repeatedly touched the couch and a cushion in a way that suggested considerable tactile sensitivity.

He told me of his fascination with hoaxes, crackpots in science and the ostracized scientist, and more generally controversy in science.

"I've worked with crackpots. I used to be the specialist on angle-trisectors and the like in Chicago for a time. . . . I always wonder about the line between being a crackpot and being really original but unheard. There is no sure way. Galois, one of the greatest mathematicians that ever lived, was rejected from school because all he had to offer to get in were these crackpot theories which were absolutely correct. . . . I am in correspondence now with a wonderful old Indian gentleman who desperately needs a teaching job. He has finally got his immigration status settled in the United States. His written English is just beautiful. As a youth he formulated some antirelativistic theory of relativity. I just saw his manuscripts, beginning yesterday . . ."

Yet on an earlier occasion, when we had been discussing some successful established scientists, he expressed a very different attitude toward some of them.

"I rarely can take 'big geniuses' seriously. I am always crossing people who think, whether rightly or wrongly, that they have some really great discovery . . . some of those people might take me into their confidence and I begin to see that they consider me as bright as they are, and then I am even more upset than I was when I thought they were arrogant. I am a lot older now, and I see that a lot of these people are just naïve."

Now he candidly volunteered a more personal concern, perhaps not uncommon among successful innovators: "I hold a minority view (in statistics), I hold it quite rigorously and narrowly . . . I have sometimes wondered, how do I know that I am not a crackpot?" In strongly espousing a viewpoint in statistics, he was going well beyond what can be rigorously proved by mathematics or refuted by experiment. He further reflected on the fact that in spite of his minority view, which had grown increasingly radical, he had not been ostracized by the statistics community. On the contrary, the viewpoint he had first introduced in the United States twenty years ago has won many adherents.

Savage spoke of encountering people—both professional statisticians and those in quite different professions—who naturally, habitually think along lines of personal probabilities, utilities, Bayesian statistics. It seems more like a personality trait than the result of intellectual training. Savage described the reassuring experience of encountering someone who is viscerally and intellectually attuned to his own outlook.

"We are far apart geographically and far apart in our other scientific interests, but when we talk we know that we agree about almost everything. There is a system there that works. It's as though you had been taught in childhood what was supposed to be Albanian, but you never knew an Albanian and you weren't quite sure whether you'd been tricked. But if you got off the dock in Albania you would know. . . . It's like that. I know that it's cogent because it's in such absolute detailed agreement with Schlaifer."

The growth in acceptance of personalistic statistics in the United States was nurtured by Savage's vigorous and patient didactic efforts. In conversation about his work, as in his writings, Savage showed a constant awareness of who the listener or reader was; he did not try to impress the reader, but conscientiously helped him to understand each step, besides trying to persuade him of a point of view. In the preface to his book, which is not a textbook but a presentation of Savage's new formulation of statistics, a mixture of formal mathematics and homey examples, he nevertheless offered the following pedagogical advice:

> If one wants only the gist of it (a long mathematical argument), he may read such material once only; but otherwise he must expect to read it at least once again. Serious reading of mathematics is best done sitting bolt upright on a hard chair at a desk. Pencil and paper are nearly indispensable; for there are always figures to be sketched and steps in the argument to be verified by calculation.[38]

Savage was an able polemicist and writer of English prose, good humored, respectful and courteous toward opposing views, disarmingly candid about the difficulties of his own view, and relentless about the difficulties of alternate viewpoints. Thus section 4.2 of his book is devoted to those criticisms of his viewpoint that he admittedly finds difficult and confusing to answer. Again, in an essay he read to the American Philosophical Association in 1967, thirteen years after the book was published, he said: "We who defend the personalistic view naturally consider some of the most striking difficulties raised in objection to be illusory. I shall talk here not about those difficulties with the personalistic view that I can resolve, at least to my own satisfaction, but about those that particularly puzzle or confuse me. Consequently, I shall be raising many vague questions and making relatively few clear statements."[39] The vague and puzzling aspects of which Savage refers are not in the formal mathematics but in the philosophical foundations of his normative theory. (Since Savage's death one serious difficulty with his personal statistics has arisen from experiments that show that most people's utility functions and personal probabilities depend strongly on the words used in stating facts and alternatives, not only on the facts and alternatives actually presented.)

Savage's book on statistics is dedicated to his father. If any area of scholarship would be meaningful to a businessman, I suppose it would be the problem of how to make sound decisions in the face of uncertainty. Of course it's not the same as coming into the business.

Gregory Bateson, in contrast to Jimmie Savage, grew up in a world where intellectual preoccupations were commonplace—especially the large ideas around the Darwinian theory of variation of species. He was heir to a kind of intellectual aristocracy. The countryside around Cambridge and the world of creatures to which it was hospitable, so different from the industrial urban Detroit environment, provided the favorite pastime for Gregory as a youngster, his brothers, and their friend G. Evelyn Hutchinson. As Hutchinson recalled,

> Every autumn one could find the large fawn-and-chestnut-colored caterpillars of the goat moth crossing the road, looking for sites into which to burrow and become chrysalises. . . . Equally exciting were the bizarre caterpillar of the puss moth, the various species of hawk moth—poplar, line, and eyed—and the great green metallic musk beetle that sometimes was seen sitting on arrowhead leaves growing from backwaters of the river . . . adjacent rough pastures with ditches and ponds yielded newts and sticklebacks; occasionally one saw the kingfisher, the most spectacular of all ordinary British birds, darting along a stream . . . Daubenton's bat used to sleep under the spans of the bridges and hawk gnats up and down over the water at dusk. . . . Much more obscure, and in some ways more fascinating, were the animals that one knew some learned men studied, for the town was full of amateur naturalists as well as having its academic professional zoologists. . . .[40]

Gregory thirty years later brought his boyhood friend Hutchinson to join him at the cybernetics conferences.

Gregory's style as anthropologist was informed by the habits of a trained naturalist, habits and pleasures of observation. His style of intellectual understanding, which he could adapt from one discipline to another, was primarily ecological, long before "ecology" had become a popular notion. At a conference in the 1950s, when asked to speak about himself, he talked informally:

> . . . I suppose the thing I hold sacred is something in the nature of patterns, and that which makes me angry is the violation of patterns in some form or other. So I find myself today studying schizophrenic subjects, trying to help them find valuable patterns in their lives; and on the other side of the same picture, I find myself angry at the distortions of pattern that happen to them as children. . .[41]

But thinking in terms of patterns and ecological stability, as Bateson did, led from concrete observation to reflection on larger systems. Elucidation of general principles, abstract language, was needed to make sense out of nature's diversity. For his own early notions of proto-learning, deutero-learning, and higher order learning,[42] i.e., levels of learning, he sought a still more abstract and widely applicable formulation; he found it at the cybernetics conferences by adopting the language of the Russellian "theory of types" as a metaphor. Similarly, for comprehending his early observation of stability in the Iatmul culture,[43] his own concepts of "symmetrical schismogenesis" and "complementary schismogenesis" seemed too narrow, and Bateson adopted the language of cybernetics pertaining to circuits with positive or negative feedbacks. General and abstract principles helped him to understand how the world worked and especially permitted him to see mind, culture, and biology as a natural unity. He addressed and stayed with the issues that mattered to him, rather than emasculating the questions so that they might be answered neatly in terms of available scientific techniques. Thus he often found himself an intellectual pioneer, confidently and resourcefully surveying the wilderness. Far more directly than for most scientists, science for Bateson was a means for general understanding and wisdom. To Bateson wise action required first of all deep and rich understanding, a going beyond what the academies, with their departmentalization, recognize as learning.

I interviewed Bateson at the Oceanic Institute in 1968, spending most of a day with him.[44] He spoke in complete sentences, highly intelligent and articulate. Yet he seemed to be making an effort to describe his recollections accurately rather than merely indulge reminiscences. . . . His tone of voice was authoritative, implying naturally, quite unassertively "this is how it was" or "this is how it is." From time to time a particular memory would cause laughter to well up—a gentle, earthy and voluptuous laughter, that seemed to express delight and appreciation of the human comedy in its many-fold subtleties.

One of the members of Bateson's graduate seminar twenty years earlier evoked in his description qualities I had seen in Bateson in a different setting:

> . . . Bateson taught us to see the symbolism, like nerve and muscle, beneath the skin of ordinary appearances. He noticed everything, as if seeing was a marvelous game. The Iatmul culture of New Guinea, which he analyzed in *Naven,* seemed to fill him with joy because it was so absurdly and touchingly human. He talked about it as if he felt an immense relief that men and women somewhere in the world should have clear-cut and splendidly eccentric customs. He took a special delight in the men's ritual boasting in the ceremonial house, as if he thought it both comical and natural that men should boast.[45]

This humor-filled and appreciative understanding of things human would have to be corroborated by any codification of wisdom, if such codification were to be convincing to Bateson. It is somewhat ironic that he reached for the formal logic of the *Principia Mathematica* of Russell and Whitehead, as mediated to him through the mathematician and student of Russell, Norbert Wiener, at the Macy conferences, as a metalanguage suited for his purposes.

From that first conference in March 1946 when Wiener had discussed Russellian paradox and the computer's oscillatory (yes-no-yes-no-yes-no) response to it, Bateson found considerable rapport with Wiener.[46] They had in common some uncommon preconceptions about science that influenced their scientific styles: both were highly interdisciplinary in their range of curiosity and had a sense that everything is connected to everything else, and that such connections can be manifested on an abstract or philosophical plane. Unlike Savage, neither agreed with the prevailing logical positivistic philosophy of science. Both found highly abstract, cross-disciplinary principles interesting, although they knew, as Wiener put it, "as a rule 'high' order, very abstract and general statements are not amenable to experimental test. They have to be broken down into more specific terms."[47] Wiener and Bateson were both willing to translate exact theorems of communication engineering, physics, and formal logic into relatively loose verbal, formal statements—which they would then extend and apply in a heuristic way to other areas of science, although most scientists frowned on such practices. Bateson in particular applied them to the human sciences. Having accepted the legitimacy of the human endeavor to understand the world in a more than piecemeal, "departmentalized" way, they had necessarily to take into the bargain paradoxes, incompleteness, vagueness, and tentativeness. Narrow specialization had been the sine qua non of science for generations; consequently, Wiener's and Bateson's efforts to describe the world and ourselves in a comprehensive, holistic way and yet function as scientists were not taken seriously by colleagues and tended to isolate them.

Their collaboration, which took place through visits and correspondence, suggests that social scientists and mathematicians may be able to work together if they have common assumptions about science. Savage and Bateson ignored each other's work. Had either attempted to impose his own method of thought on the other's work, the other's weaknesses would have been pointed up. At the time of the Macy meetings, when each was busy developing his own constructive ideas, such criticisms could only have hindered them. Thus the mutual lack of interest was fortunate for both. It tends to support the birds-of-a-feather thesis that the only Macy group social scientist with whom Savage collaborated was sociologist Paul Lazarsfeld. Lazarsfeld, the director of the Bureau of Applied Social Research of Columbia University, favored a positivistic philosophy of science as Savage did. He had been a champion of quantitative and statistical sociology, and his institute was a beneficiary of the generous financial support that flowed into mathematical social science in the United States in the late 1940s and early 1950s. Savage was helpful to him over the years in some statistical problems.[48] Like Savage, Lazarsfeld did not find the Macy conferences particularly relevant to his own work. He had been absent from the sixth meeting and was subsequently dropped from the roster.[49]

Bertrand Russell's theory of logical types arose in the early part of the twentieth century as a result of contradictions and paradoxes in the mathematical theory of infinite sets. For example, the concept of "the class of all those classes that are not members of themselves" was self-contradictory; one such contradictory entity within mathematics endangered the self-consistency of all of mathematics, so that Russell came to the conclusion that all such paradoxical statements had to be ruled out. He devised a "theory of types," according to which an individual, a class of individuals, a class of classes, etc., form an ascending hierarchy—and to avoid paradox, one must remember that the legitimate members of a set at one level all belong to the level just below. The classic Greek paradox about the barber who shaves all those men in his village, and only those, who do not shave themselves, led to contradictory conclusions as to whether the barber shaves himself. According to Russell the confusion arises because the statement speaks of a class (shaver) that is or is not a member of itself.

A parallel consideration of language leads to a corresponding hierarchy of simple statements, statements about statements (metastatements), statements about metastatements, and so on. To avoid paradox, each statement may be about only elements at the next lower level. The paradoxical assertion "This statement is a lie" is ruled illegitimate because it is a statement about itself rather than about elements at a lower level in the hierarchy. After 1946 Bateson increasingly used the Russell theory as a heuristic device and metaphor for describing human communication.[50] In the years following the Macy conferences, he employed it for a general theory of play and fantasy as well as for the double-bind hypothesis in schizophrenia.

Bateson's originality in using the theory was already evident in his provocative presentation at the ninth Macy conference on humor in human communication.

> One of the rather curious things about *homo sapiens* is laughter, one of the three common convulsive behaviors of people in daily life, the others being grief and orgasm. . . . Of the three types of convulsions, laughter is the one for which there is the clearest ideational content. It is relatively easy to discuss what is a joke, what are the characteristics that make a joke, what is the point of a joke. The sort of analysis that I want to propose assumes that the messages in the first phase of telling the joke are such that while the informational content is, so to speak, on the surface, the other content types in various forms are implicit in the background. When the point of a joke is reached, suddenly this background material is brought into attention and a paradox or something like it is touched off. A circuit of contradictory notions is completed.[51]

Bateson argued that the "paradox" in humor is the same as that which arises from mixing of logical types and presented the hypothesis "That the paradoxes are the prototypic paradigm for humor, and that laughter occurs at the moment when a circuit of that kind is completed." Unfortunately, Savage missed that particular conference and both Wiener and von Neumann had dropped out just prior to the ninth conference; their responses would have been interesting. In the ensuing discussion Bateson entered into the spirit of the cyberneticians and suggested that one might construct a cybernetic circuit that would oscillate in response to certain types of contradictions. This idea, a machine with a sense of humor, was immediately taken up by others. Bateson also noted one important role of humor in the relation of people to one another: "It gives the persons an indirect clue to what sort of view of life they share or might share"; and went on to examine the pattern of interaction in such a process.

Although the discussion, especially comments by Walter Pitts, showed Bateson's theory of humor to be only incompletely worked out, he was eager to describe his basic thesis, which goes beyond an analysis of humor:

> I should like to present to you the notion that these paradoxes are the stuff of human communication. As scientists, we try very hard to keep our levels of abstraction straight; for instance, in the conferences we have gotten into very great trouble when the levels of abstraction became tangled and the theory of types showed itself. In ordinary life, as distinct from scientific talk, we continually accept implicit paradoxes. . . . The freedom to talk nonsense, the freedom to entertain illogical alternatives, the freedom to ignore the theory of types, is probably essential for comfortable human relations. . . . I am arguing that there is an important ingredient common to comfortable human relations, humor and psychotherapeutic change, and that this ingredient is the implicit presence and acceptance of the paradoxes. . . . The alternative to the freedoms introduced by paradox is the rigidity of logic.[52]

Bateson later insisted that an awareness of the many levels of reality and a sensitive appreciation of the contradictions and paradoxes they create is crucial to making wise interpersonal, social, and political choices. In this connection he liked the analogy to the task of backing up a truck with several trailers attached, and the difficulties encountered when one turns and the successive trailers move in contrary direction.[53]

Bateson strongly criticized some applications of von Neumann's game theory by visualizing it on a different conceptual level than Game Theory itself.[54] It ignores the quintessentially human and leaves no room for learning, in particular, disallowing deutero-learning. Savage's decision theory depends on practically the same "utility" concept as game theory and is open to the same criticism. Further, the premises of game theory propagate changes that Bateson suspected "are in a paranoidal direction and odious" (the latter comment would apply to Game Theory but not to decision theory). Wiener seconded Bateson in using the theory of types as a metaphor to criticize applications of game theory: you can win the battle and lose the war; and you can win the war and lose everything that is dear to you.[55] McCulloch also disagreed with the utility concept of Savage and von Neumann. In a little confrontation at the eighth meeting between him and Savage, McCulloch simply characterized the utility concept as "illusory," because he regarded "value" as multidimensional, "with very little chance of its being simplified to a measure." Savage's decision theory and Bateson's approach to good choices may be viewed as orthogonal to each other: Savage worked out the techniques in rigorous detail on one level, whereas Bateson emphasized the necessity of considering many levels simultaneously; but in Bateson's formulation the rigor and clarity of Savage's model are missing and the techniques for arriving at good decisions are not fully spelled out. Bateson could view the Savage theory from the outside, in the context of a particular kind of decision to be made, and ask whether the utilitarian and quantitative assumptions were suitable for the choice in question. Bateson's disagreement with the use of statistical decision theory as a generally applicable approach is reflected in his later insistence on "the obsolescence of the anti-aesthetic assumption . . . that all phenomena (including the mental) can and shall be studied and *evaluated* in quantitative terms."[56]

Bateson's most explicit discussion of "making choices" is found in a 1968 paper presented to a Wenner-Gren Conference, where he discusses different categories of learning.[57] Learning I serves to correct errors of choice within a set of alternatives, whereas Learning II, which is synonymous with "the acquisition of world views," results in corrective change in the set of alternatives from which a choice is made. The phenomenon of Learning II, according to Bateson, involves changes in which the "stream of action and experience is segmented or punctuated into contexts together with changes in the context markers." For example, certain meditations or psychotherapies may result in a corrective of a person's habitual inability to entertain seriously certain choices and thus increase the options available to her or him.[58] This is an example of Learning II. In political decisions as well, everything depends on how the various options are framed. Thus the decisive battle between the Democratic and Republican candidates for office is typically over which of them succeeds in defining what the issues are. "The outcome of the game of politics depends on which of a multitude of possible conflicts gains the dominant position," although the unaware voter usually just chooses sides in a conflict that has been decided on by others.[59]

Savage's prescription for making choices encourages and essentially corresponds to Learning I. It does not concern itself with higher-order learning, even though such learning could turn all evaluations on their heads. Although in his later philosophical reflections Savage sought to identify some problematic issues concerning his theories, he was not even close to coming to terms with Bateson's concerns.[60] Nevertheless it is noteworthy that Savage did wonder how to understand a person's feeling of compulsion to "correct" his or her own circular preferences when confronted with them. He also wondered how to incorporate into theory, or if it is a source of paradox how one is to judge, the worthwhileness of expenditure of a given amount of money to make an "optimum" economic decision.

At the Macy conferences the distinction between a probability space with fixed options and the alternative consideration of the possibility of enlarging the space of possible occurrences had arisen as a source of controversy in another connection, namely, with two different definitions of "information," one favored by the American engineer Claude Shannon, a frequent guest at the meetings, and the other championed by a visitor from the British Isles, Donald MacKay. Both were at the eighth conference, and in the lively debate that followed MacKay's presentation Savage was an active participant.[61] (Bateson did not attend that conference.) Shannon's concept of "information" arose in connection with communication engineering and the problem of specifying the channel capacity required to transmit "information."[62] From the engineering point of view it is irrelevant whether the messages have any meaning whatsoever. Heinz von Förster's criticism applies, that the anthropomorphic notion of "information" is misplaced here; nevertheless, it had become the accepted word in communication engineering. The great merit of Shannon's and also Wiener's equivalent definition of information is that it lent itself to proving a number of rigorous mathematical theorems. MacKay also took into consideration the new information obtained in a scientific experiment, which might not be included in the space of possibilities envisioned prior to the experiment, and required an expansion of that space. But beyond this, he was interested in expanding the concept of information to encompass "meaning." In any case MacKay's concept of information included Shannon's as a special case, which he called the "selective-information-content." His broader definition included as well what he termed "structural-information-content" (reflecting the number of independent features that are to be specified) and "metrical-information-content" (reflecting the number of units of evidence required for the description).[63] MacKay's later philosophical reflections on probability, meaning, mechanism, and information are given a special character by the circumstance of his religious piety and the explicit effort to reconcile it with the Bible and his Calvinistic faith.[64]

Bateson's use of the notion of logical types is an artifact. He used the concepts of formal logic to show that logic is not suitable, yet he was able to get considerable mileage out of that unsuitability. By implication, ordinary or literary language seems more suited to questions of wise choice than does the language of logic, and he might have come to that positive conclusion. He emphasized that wisdom consists of knowing the larger interactive system in which exponential changes can be initiated when choices are made. He was specifically aware of the complexity of the consequences of actions: short term and long term, for an individual as well as for the larger ecological system, cultural as well as psychological and biological, circularly and linearly causal. He had considerable success reconciling detailed scientific research with efforts at comprehensive, holistic understanding of himself and the world, an "ecology of mind"—which in his terms meant encompassing the tradition of Heraclitus and William Blake as well as of Newton and Darwin.

Bateson's view was the traditional one that wisdom resides in a (person's) mind. He was not so concerned with managerial decision making, for his was not the world of politics or business. He disliked power over people and manipulation of people, and his focus was on knowledge and understanding. Only when he thought that the powers-that-be were engendering ecological disasters did he seek to influence their awareness of the dangers. For Savage, on the other hand, wisdom resides in a technique, and he provided us with a prescription for good decision, which anyone who knows precisely what he or she wants can use.

In our complex world it is a great relief that learned men are willing to tell us practically how we can make purportedly good decisions. Interpreters of the I Ching have served a similar function in China and astronomer/astrologers did so in earlier centuries in the West. Mathematical decision theory has found resonance in business schools, the business sector of the economy, and in public administration, as well as with social scientists concerned with good design of experiments. It is well suited to bureaucracies because in spite of "personal probability" it can reflect the objectives and judgments of an administrative entity and depersonalize and mechanize the decision process. The wisdom of diffusing responsibility, often desired in organizations and readily achieved by a relatively mechanical decision process, is open to question from the perspective of the larger society. For policy and military decisions the issue has been treated elsewhere in connection with game theory.[65] A facet of mechanizing medical diagnostic decisions (and similarly choices about cures), is that "as the physician makes greater use of the technology of diagnosis, he perceives his patient more and more indirectly through a screen of machines and specialists; he also relinquishes his control over more and more of the diagnostic process. These circumstances tend to estrange him from his patient and from his own judgment."[66]

Bateson spent his life exploring new territory. He asked the questions that seem to matter most, eschewing the limitations imposed by convention and academic institutions. It is not surprising that he increasingly focused not on administration but on teaching, and that in those years—especially the 1960s—when he was outside of the mainstream (even outside of his former "social science tribe"), young people found he had something to say to them. One of them wrote the preface to

Steps to an Ecology of Mind, and characterized Bateson's work thus:

> I believe that this is a very important book . . . especially for those of my generation born since Hiroshima—who are searching for a better understanding of themselves and their world. . . . Intellectual activity—from science to poetry—has a bad reputation in my generation. The blame falls on our so-called educational system, which seems designed to prevent its victims from learning to think, while telling them that thinking is what you do when you study a textbook. Also, to learn to think, you must have a teacher who can think. The low level of what passes for thinking among most of the American academic community can perhaps only be appreciated by contrast with a man like Gregory Bateson. . . . This book is a sample of the best thinking I have found. I commend it to you, my brothers and sisters of the new culture, in the hope that it will help us on our journey.[67]

6

Problems of Deranged Minds, Artists, and Psychiatrists

It is thought that Tuke and Pinel opened the asylum to medical knowledge. They did not introduce science, but a personality, whose powers borrowed from science only their disguise, or at most their justification. These powers, by their nature, were of a moral and social order; they took root in the madman's minority status, in the insanity of his person, not of his mind. If the medical personage could isolate madness, it was not because he knew it, but because he mastered it; and what for positivism would be an image of objectivity was only the other side of this domination.[1]

Michel Foucault

A stimulating social history of the complex human phenomenon of madness was presented in broad strokes by Michel Foucault in 1961, and has inspired a variety of new investigations into the recent history of insanity.[2] Foucault described, within a social context, changing notions of what kind of phenomenon madness is and changing practices in Europe, from the sixteenth to the nineteenth century, in dealing with "deranged minds."

Some historians of psychiatry (a discipline whose premise is that something must be done about crazy people) have found it useful to distinguish three types of "models" of madness. According to the first model, insanity represents a biological problem, and therefore medical and pharmaceutical treatment is in order. The second viewpoint sees it as a psychological difficulty, and crazy people are to be treated by kindness, "moral therapies," "talk-therapies," and such. The third model places the causes of insanity on social and political conditions, and the remedy—and preventative—is a change in these conditions not only in the microcosm of an asylum but in the society at large.

Some have argued that an historical analysis going from 1800 to the present shows definable eras of psychiatric practice, in which one of the three models predominates. Moreover, one era is followed by a new era in which another of the three models has sway, and so on. Thus the biological, the psychological, and the social models succeed each other as predominant in cyclic order.[3] The vogue characteristic of a particular time can be related to the general cultural and political conditions of that time. While Foucault's work had already shown that these three models do not exhaust the possibilities, and they are not always clearly separate from each other, they are nevertheless useful. The cyclic rather than linear patterns, as well as Foucault's investigation, have challenged the vision of scientific progress in the treatment of madness. I see it as a dialogue with an ever-changing center of gravity, part of a larger discussion of the human and the social, which will be concretely manifested through the prevailing praxis, at least among the affluent. The treatment of people who are simultaneously poor and crazy is conditioned heavily by extraneous factors.

Our concern is with some differences in view during the narrow time span of a few years on either side of 1950. The people at the cybernetics meetings concerned with psychiatry came from what we have called diverse "tribes" and loyalties. One was the fashionable New York psychoanalyst and pillar of the conservative Psychoanalytic Institute in New York Lawrence Kubie, who was also a theoretician within the psychoanalytic framework and believed in psychoanalysis.[4] His primary "cluster" was the members of the New York Psychoanalytic Institute, a tight, ethnocentric tribe with more than its share of infighting and backbiting. On the other hand McCulloch, who opposed the practice of psychoanalysis, was not only in the "cybernetician" tribe but was identified primarily with neurobiological researchers. The argument between Kubie and McCulloch was complex, as was the professional relationship between them. Molly Harrower, a friend of both men, was less doctrinaire than either. A clinical psychologist, her professional identity was neither that of a neuropsychiatrist nor that of a psychoanalyst. Gregory Bateson was just becoming interested in psychiatry at the time of the Macy conferences and was beginning to forge his own views, relying heavily on concepts from cybernetics. Klüver, who was in McCulloch's camp in the controversy, was the odd man out who saw social and political arrangements as the root cause of madness. From among those advocating psychoanalytic or humanistic psychotherapies, Frank came closer than most to seriously acknowledging the role of external social and political conditions. Making abstract controversy concrete, we will have occasion to allude to all these people's attitudes toward the gifted youngest conferee, Walter Pitts, whose state of mind, a few years after the end of the conference series, deteriorated to such an extent, and whose behavior became so strange, that he would ordinarily have been classed as psychotic. But in view of his extreme brilliance as a scientist, doubt remains whether such a common classification—rather than, say, mere "eccentricity of genius"—is appropriate.

It is not surprising that in the politically conservative fifties models emphasizing the need for social and political change were not prominent. In earlier periods the social-political model was commonly accepted. In the decades prior to the American Civil War, even though insanity was held to be a disease of the brain, the primary cause was generally thought to lie in social patterns and structures. Asylums were built to provide favorable environments. Specifically, mental disorders were seen as "part of the price we pay for civilization."[5] As one historian wrote,

> Before the Civil War, practically no one in the United States protested the simple connection between insanity and civilization. Despite the tenuous quality of the evidence, Americans accepted the conclusion without qualifications. . . . Medical superintendents' explorations of the origins of insanity took them into practically every aspect of antebellum society, from economic organization to political and religious practices, from family habits to patterns of thought and education. . . . The style of life in the new republic seemed willfully designed to produce mental illness. Everywhere they looked, they found chaos and disorder, a lack of fixity and stability. The community's inherited traditions were dissolving, leaving incredible stresses and strains. The anatomical implications of this condition were clear: The brain received innumerable abuses, was weakened, and inevitably succumbed to disease.[6]

Later in the century, when mental hygiene became a concept, a leading practitioner, George Beard, "declared that neurasthenia, a disease thought frequently to lead to insanity, was a product of nineteenth-century American civilization. . . . Not every physician accepted (his) list of causes—he emphasized the printing press, the railroad, the steam engine, the telegraph, and increased mental activity of women."[7] (The most substantial analysis relating psychopathology to social structures was carried out only late in the nineteenth century by Emile Durkheim in France, in connection with his methodologically exemplary studies of suicide.[8])

The biological, psychological, and social are intricately connected, and even if social conditions are considered responsible for insanity, individuals deemed crazy must be dealt with somehow. Those responsible for treating deranged minds in the first half of the nineteenth century sought some scientific basis for treatment. It was a period of reform in medicine, education, the treatment of criminals, and the treatment of the insane. Much of the scientific justification of the reforms concerning madness came from the new "science" of phrenology.[9] The social and historical role of phrenology parallels that of psychoanalysis a century later. Like psychoanalysis, phrenology purported to provide a comprehensive scientific explanation of the origins of individual psychological characteristics. Phrenology also illustrates how right and wrong ideas can combine to form a whole, and that practical conclusions can be drawn from ideas that may have little scientific validity.

The scientific hypotheses that the brain is the organ of the mind and, more specifically, that characteristics of human personality are localized within the brain, so that "each particular cerebral part, according to its development, may modify, in some degree, the manifestation of a particular moral quality, or intellectual faculty,"[10] were fundamental to nineteenth-century "phrenology" (literally, "discourse on the mind"). The theory was developed late in the eighteenth century by a skilled Viennese neuroanatomist, Franz Joseph Gall, who pioneered the empirical study of cerebral localization. Both his conclusions drawn from observation and the "science" based on those conclusions were controversial, especially Gall's assertion that the external bumps on a human skull could reveal the development of the various cerebral parts and, thus, the character traits of an individual. Although this was the least convincing aspect of the theory, it generated the greatest popular interest. By the middle of the nineteenth century new studies in psychophysiology had shown many of the features of Gall's theory to be incorrect, and phrenology was increasingly dismissed as a pseudoscience. Nonetheless, Gall's emphasis on the localization of functions within the brain came to dominate nineteenth-century research in neurobiology, and his ideas are central to any serious history of the field.[11] Moreover, phrenology strongly conditioned psychiatric practice in asylums and was an important aspect of popular efforts at personal development, self-improvement, and the amelioration of social conditions in the United States and Great Britain.[12]

Although one can make historical analogies between the roles of phrenology and psychoanalysis (the latter represented by Kubie at the meetings), its link to laboratory neuropsychiatry—Warren McCulloch's interest—is even more direct, transmitted from one generation of researchers to the next. Gall had "convinced the scientific community once and for all that 'the brain is the organ of the mind' and argued strongly that both its structure and function could be concomitantly analyzed by observation rather than speculation."[13] Phrenologists were hailed as having overcome the mind-body dualism of Descartes by combining the study of mind with neurobiology. In this respect they also resemble those, such as McCulloch, who aimed to describe the "embodiments of mind" in the brain.

Phrenology played a significant role in the politics of insanity. In the eighteenth century the treatment of insanity was based primarily on the premise that it had a medical, somatic basis. But the early nineteenth century brought a new approach, that of "moral treatment," which emphasized kindness and compassion. Since these were not the exclusive turf of the medical profession, moral treatment might have been opposed by physicians. Although phrenology favored the exercise and development of underdeveloped faculties and advocated specific "moral treatments" as cures, it defined insanity as a disease, giving a scientific legitimation for somatic treatments. And because phrenology required the therapist to explore carefully the proclivities, psychological strengths and weaknesses, and personalities of each individual prior to treatment, it was a tool for devising an individually tailored course of moral treatment under medical auspices.[14] Many nineteenth-century American asylum superintendents became proponents of phrenology,[15] although after the Civil War medical and surgical treatments were increasingly used. The view of insanity as a disease eventually became conventional. It was still conventional at the time of the Macy meetings, although Gregory Bateson, as we shall see, expressed skepticism about it.

Phrenology was also an outstanding example of a generally optimistic philosophy of self-improvement—"Since, by measuring a skull, one could analyze a character, determine its weaknesses and correct them, ascertain its aptitudes and encourage them, the ramifications of phrenology seemed limitless."[16] In the pre–Civil War period Horace Mann and Gridley Howe brought phrenology to bear on educational reform, and others used it in prison reform. In education, for example, phrenologists emphasized the need for understanding of the characteristics of each child and gearing the training accordingly. The phrenology literature claimed that it provided self-knowledge, giving increased freedom of choice and greater capacity for personal development. The phrenological reform movement advocated child-rearing practices that relied on persuasion and affection rather than compulsion, and frank discussion of sex replaced "morbid delicacy or prudish affectation." The style and message of its leading exponents in the United States—reminiscent of Frank's enthusiastic persuasiveness—was on the whole progressive, generous, and optimistic. Nevertheless, a doctrine that judged people's moral character by the shape of their skulls was susceptible to being used to bolster racist views and to support some mean and malicious actions.

The Macy conferences were one brief moment in a long and many-faceted discussion about the nature of insanity. When they took place, psychoanalysis was still in its heyday; prefrontal lobotomies were carried out in some mental hospitals to "cure" psychopathologies, shock treatment was widely practiced, and hydrotherapies were popular. In the chapter on wise decisions we saw that those with opposing views mostly ignored each other. In the case of psychiatry, however, there was an overt cross-disciplinary controversy. The manner in which the controversy was conducted, especially between Kubie and McCulloch, mutual friends who prided themselves on being scientific, is a historical datum: It suggests very strongly that within psychiatry something more than the nature of the scientific evidence or the cogency of theories is at issue.

Kubie and His Controversy with McCulloch

Lawrence Kubie was one of the first psychoanalysts educated in the American medical school tradition and, like Freud, began his career with experimental researches in neuropathology, which few American psychoanalysts did. His psychoanalytic training took place not in Vienna or in Berlin but with the English analyst Edward Glover, in England. Kubie was exceptional as well in his broad advocacy of psychoanalysis as of benefit to humankind. His identification with the rigors of the New York Psychoanalytic Institute (he was for a time its president and, in 1936, dedicated a book to the institute) is reflected in his concern for distinguishing "between valid psychoanalytic procedures and practices which masquerade under its banner."[17] He was a guardian of the orthodox analytical establishment, which was increasingly on the defensive against rival schools that threatened to undercut its influence. Rivalry among psychotherapeutic schools, often characterized by acrimony and self-righteousness, had long been part of the profession: It was a precedent set in Freud's quarrels with his erstwhile disciples who had set off on divergent paths. Although he was a warm and friendly person, Kubie's appraisal of leading members of competing schools was also not always generous. After Harry Stack Sullivan's death, Kubie described the leader of the rival interpersonal school as "an empty shell of pretensions without substance."[18] He confronted Carl Rogers, a major figure in humanist psychotherapy, directly, expressing hope that his original impression was mistaken: "You sounded to me as though you fail to comprehend even the most elementary principles of modern psychotherapy as practiced by analytically trained psychiatrists."[19] In later years he continued to disparage Rogers. He administratively punished and later made uncomplimentary comments about the neo-Freudian Karen Horney, describing her as "someone confused and essentially trivial and transitory."[20] Horney was a particular thorn in Kubie's side: some of her ideas were at variance with Freud's, and she was one of several who, after leaving the institute in 1941, went on to form the rival American Institute for Psychoanalysis.[21] In contrast to the direct human relationship between therapist and patient advocated by the humanist, neo-Freudian, and interpersonal schools of psychotherapy, the austere Freudian tradition prevailed at the institute. The technique involved a patient free-associating while lying on a couch unable to see the seated analyst: "By remaining as far as possible a dummy in a store window on whom the patient drapes his fantasies, the analyst becomes a screen on which the patient projects the shadows out of his own past. The analyst cannot serve these uncovering purposes if he presents himself to the patient as a real human being."[22]

Kubie was also distinguished by his particular concern with the psychoanalysis of creative people—artists, writers, scientists—and by his efforts to establish psychoanalysis as a science. Although some of his strictly Freudian colleagues considered him a gadfly within the profession, within the world of psychoanalysis, his position was essentially "orthodox" rather than "reform." At the time of the conferences he was in private practice in New York and also teaching at Yale University Medical School.

Born in New York City of Jewish parents, his father an importer of crude rubber, Lawrence Kubie was the youngest of three children. He received his medical education at Johns Hopkins University School of Medicine, and subsequently became a member of its staff. He was first associated there with psychiatrist Adolf Meyer and subsequently with laboratory neurophysiologists. He continued to work in experimental neuropathology at the Rockefeller Institute and, from 1928 to 1930, as a U.S. National Research Council Fellow in London, where he was in contact with Sherrington, known for his pioneering research on reflexes and the integrative action of the nervous system. In 1930 Kubie, with encouragement from Sherrington, published an article in which he conjectured about the neurological basis of spontaneous involuntary movements, as, for example, epileptic fits or the scintillating visual phenomena associated with migraine. He suggested that

> one simply picture the central nervous system as a place in which, under certain conditions and in certain areas, excitation waves move along pathways which ultimately return them to their starting points. This possibility is selected because such a circular wave would constitute a source of energy, which under certain conditions would give little or no outward sign of its existence, but which, with a slight change of conditions, might suddenly become manifest.[23]

Reverberating circuits of neurons, which in the 1930s were observed and studied in the laboratory by Macy conferee Rafael Lorente de Nó, were also prominent in Pitts's and McCulloch's later ideas about cognitive processes in the central nervous system. Kubie's 1930 article had been important to them, and McCulloch billed Kubie as a pioneer who had proposed circular paths. A decade after his article in *Brain*, Kubie had revised and extended Freud's notion of the repetition compulsion and had come to view involuntary repetitiveness as the core of all neurosis or psychopathology, and circular neuronal paths as its physiological basis.[24] In this way he linked the neurophysiological laboratory with the psychoanalytic couch.

Kubie's "conversion" to psychoanalysis—and it does have the flavor of a religious conversion—took place during his London years, while he himself was undergoing psychoanalysis. The groundwork for the change in career, however, had been laid earlier. His early work under Meyer, who viewed mental illness not only in biological terms but also as a personal reaction to social and life situations, had in a sense prepared him. Kubie's shift from laboratory scientist to psychoanalyst was a major event, on which he reflected over the years. As the Macy conference series was coming to a close, he recalled that when he was a young scientific researcher known to have psychiatric training, "young colleagues and sometimes older ones would drift in to talk, not about scientific issues but about their personal problems." It became clear to him then that "a scientist's ability to endure the prolonged frustration and uncertainties of scientific research depends on neurotic components in his personality."[25] Although usually Kubie emphasized unconscious drives, in connection with a scientific career he called particular attention to the stresses created by the usually low salaries and economic insecurity.

Kubie pointed to gender identification as a problematic aspect of his mid-life career change:

> Early conflicting identifications with an older sister and an older brother (a powerful and naturally gifted athlete) paralleled, infused, and also confused the issue of whether to go into psychiatry or into experimental neuropathology. Psychological vs. organic had meant to me both woman versus man and internal versus external.[26]

Kubie saw the gender issue as intense, he told me, because his mother died of pulmonary tuberculosis when he was three.[27] He discussed the impact of early loss of a parent on gender identification in general terms in later writings.[28] The type of psychotherapeutic treatment he chose to give followed rules that avoided direct, personal interaction between therapist and client—a preference some orthodox psychoanalysts might view as reflecting the particular efficacy of their methods in dealing with problems of gender identification in patients who have lost a parent.

In the early 1940s Kubie was drawn into war work. He became a psychiatrist-consultant to the U.S. Army and Air Force and did studies for Army Intelligence of the influence of certain drugs on the ability of prisoners of war to stand up against interrogation.

His theoretical formulation within psychoanalysis included a rejection of the Freudian structural concepts of id, ego, and superego, and an explanation of pathologies solely in terms of unconscious fears and desires. But like Freud, Kubie saw the unconscious as a source of tyranny and the preconscious as a source of freedom. He also saw the third member of the tripartite model, the conscious mind, as crucial in overcoming the tyranny of conflicting unconscious drives.[29]

Kubie came to the conferences commended by Fremont-Smith, whom he had known since they both had worked on the physiology of the cerebrospinal fluid in the 1920s. Both had defected from careers as research scientists. A further bond between them was Fremont-Smith's sympathy to psychoanalysis. McCulloch, on the other hand, was primarily interested in Kubie's early work on circular paths in the brain. McCulloch had posed himself the question, "What is a man that he may know a number?"—a question that led to logical, neurophysiological, and epistemological investigation; Kubie's focus after his "conversion" was on the particular technique of psychoanalysis—a technique based on a theory that answered a different question, "What is a man that he suffers neurosis?," but did not encourage philosophical reflection on that question. Nonetheless Kubie did not wish to lose his status as a (male) scientist. He attempted to make psychoanalysis into a science and to describe it in a way convincing to the scientists at the conferences.

Kubie presented ideas from psychoanalytic theory and practice to the cybernetics group again and again with extraordinary patience, and each time generated heated controversy. The fact that Kubie in his work dealt with subjective experiences, which the neurophysiologists and mathematicians with their mechanistic models tended to ignore or belittle, suggests that Kubie's effort to persuade could be only an uphill road. The blatantly subjective nature of a psychoanalyst's perception of his or her client, in contrast to most other types of scientific observation, heightened the skepticism of empirical scientists. At the sixth meeting, Brosin spoke admiringly of Kubie for "the skill with which he has reserved judgment and shown restraint" in the discussion. One person described Kubie's unperturbably patient attitude as that of a missionary preaching to the heathen, and years later Bateson commented laughingly that Kubie defined behavior as neurotic when it is repeated again and again even though it doesn't work, but that was exactly what Kubie had done when he repeatedly and always unsuccessfully presented psychoanalysis at the conferences.[30] Alternatively, Kubie's patience with hostile comments can be construed as reflecting his psychoanalyst's habit of tolerating every kind of transference from analysands and trusting that the transference will eventually be broken. Kubie's presentations and the questions asked by some of the skeptics, which make up about one-sixth of the published conference transactions, exposed the problematic nature of psychoanalysis as science.

The physiologists in the group were willing to listen to him partly because of their respect for his earlier work, and clearly he was someone with whom one could argue. Bateson raised the question, "If there were no consciousness would there be neurosis?," to which Kubie replied that he didn't know. Bateson's question hints at a point of view nearly diametrically opposed to Kubie's. Kubie believed in making unconscious material conscious and guiding one's actions as much as possible by conscious purposes. As he said at the sixth meeting, "The degree to which any act is serving conscious purposes has a direct correlation with its essential normality, and the degree to which it is serving unconscious purposes has a direct relationship to its neuroticism."

Bateson came to view consciousness and conscious purposes as the source of, not the solution to, people's troubles in the modern world.[31] He felt that a reliance on consciousness and conscious purposes narrows possibilities and cuts off the direct responsiveness to psychic life; such narrowing has been and continues to be a cause of the destruction, by humans, of the physical environment, the balance of nature, and ultimately ourselves.

The question of norms and values in psychoanalysis was raised at the sixth conference, but much of the topic remained untouched. Kubie maintained, of course, that even though many neurotic forces are socially productive, any hope of human progress depends on preventing or limiting neurosis. Mead brought in the cultural relativity of the appropriateness of kinds and degrees of consciousness. Savage, Wiener, and Pitts attempted to disentangle the norms and values of a psychoanalysand from those of a psychoanalyst who wishes to eliminate neurosis. McCulloch differed profoundly from Kubie concerning the normative elements. McCulloch valued individuality, creativity, talent, and their products more highly than the purported benefits of psychoanalytic intrusion into people's lives, whereas Kubie wished that Vincent van Gogh and Beethoven might have had the benefit of psychoanalysis to make their lives more wholesome.[32] Neither approached the contentious issue from the viewpoint of social usefulness, as William James had in his Lowell Lectures: "We are all instruments for social use, and if sensibilities, obsessions and other psychopathic peculiarities can so combine with the rest of our constitution as to make us the more useful to our kind, why, then, we should not call them in that context points of unhealthyness, but rather the reverse."[33] No one at the conferences questioned the social and political premises of psychoanalysis.

At the seventh conference Kubie spoke on "the relationship of symbolic functions in language formation and in neurosis." It was in that context that he said "if there was no unconscious, there could be no neurosis." Pitts sought clarification about "the unconscious": "Is not your conception of the unconscious like the vermiform appendix, in that it performs no function and becomes diseased with extreme ease? It is there for no discernable purpose." After some discussion which failed to satisfy him, Pitts persisted: "Suppose one did not have one, what would happen? . . . It is not obvious to me why it is not conceivable to have a human being without one of these objects called the unconscious. How would he act and what would he do wrong?"

Pitts's and Bateson's critiques of psychoanalysis were concerned with opposite poles of the conscious/unconscious dichotomy. To Pitts the logical inconsistencies and ambiguities of psychoanalysis vitiated the credibility of the theory, and he wondered about the necessity of the "unconscious," whereas Bateson wondered whether the psychoanalytic emphasis on consciousness might not be a pathology among psychoanalysts. Neither Bateson nor Pitts was in the end persuaded by Kubie.

To researchers on the nervous system it is always crucial whether a human characteristic has counterparts in the animal world. From Kubie's insistence on the essential role of symbolic processes in neurosis,[34] (and despite his agnostic attitude as to whether symbolic processes are uniquely human) Pitts suggested that a so-called animal neurosis had nothing in common with human neurosis. And Klüver, apparently with tongue in cheek, discussed models of psychoanalytic mechanisms in birds based on knowledge of their behavior patterns. In the same spirit he proffered the possibility that Freud was subject to eidetic imagery, and that it would be amusing to regard psychoanalysis in that light. Kubie remained unruffled; he could always count on Fremont-Smith, Henry Brosin, and some others as allies in defending psychoanalysis.

Norbert Wiener had no essential objection to psychoanalytic practice, although he believed it appropriate that the theory be rewritten in terms of information, communication, feedback, and systems.[35] He stimulated Gregory Bateson to begin thinking about psychiatry in these terms.[36] Kubie was also at the time, from within the profession, criticizing Freud's "economic principle"—psychic energy viewed quantitatively.[37] As for Wiener, he preferred using his unconscious conflicts and tensions as resources for mathematical and scientific innovation, rather than dissipating them by making them conscious on a psychoanalyst's couch.[38] When he experienced intense personal stress, however, he would visit a psychiatrist for a few sessions.

At the sixth meeting Wiener asked, "Tell me this. Is the observer at a greater disadvantage as he observes something like himself?" He went on to say that in physics the measuring instrument must avoid resonances with what is being measured: "If you want to investigate blue light you don't put the blue light under a microscope that operates in blue light."[39] Kubie and Fremont-Smith explained that the very discipline and training of a psychoanalyst are intended to achieve detachment (notwithstanding the need for "transference") and avoid resonances as much as possible, and they convinced Wiener who acknowledged that no doubt "the psychoanalyst has gone as far as anyone in missing the resonances."

Perhaps having learned from his earlier difficulties at the conferences, in the final presentation Kubie did not emphasize psychoanalytic theory, but characterized himself as a naturalist observing humans and posed the problem of how to engage in scientific discourse about the phenomenon of emotions. To describe their function he adopted the idiom of the conferences:

> Although emotional states are themselves products of complex psychological processes, they are also causal in that they exercise a vitally important feedback influence on psychic processes. In this circular or feedback function, they are like the governor on a machine; indeed, this is the major key to an understanding of the role of emotions in psychic life.[40]

Do concepts such as elation, anger, emotion, have any well-defined meaning? Conferees seemed to find sharp definition problematic, whether these terms were used in a subjective report, a behavioristic description, or in terms of physiological concomitants. Kubie spoke of the desirability of finding a way to measure and quantify emotions, but Julian Bigelow—the most sophisticated mathematician among those participating in the discussion—threw cold water on that suggestion by calling it a misunderstanding of the appropriate use of mathematics in scientific description. He pointed out that it "would in fact be an impossible thing. . . . There seems to be very little likelihood that there would be any invariant measure of anger from one man to the next." Pitts and McCulloch avoided reference to "emotions" or "feelings" in all of their scientific writing, but for Kubie as for Lawrence Frank the concept of emotions seemed eminently useful. As a first step toward definition and possible quantification, Kubie and Frank agreed,

> We may have to try to state some of the dimensions . . . : Are we dealing with a process that is provoked by the immediate, present situation or by something that happened a long time ago where some feedback process seems to be operating? Are we dealing with a process that is irradiating or is it localized? Are we dealing with an expression that comes out in some sort of overt motor activity, symbolic language, or in some visceral disturbances? Is the target of expression another person, a symbol, activities, or the self?[41]

What seems to be missing in this male-scientific discussion of emotions, although it is part of the problem, is the possible validity of appraising, knowing, and even naming others' emotions on the basis of one's own experienced emotions. Can empathy permit correct understanding of and spontaneous response to another person's emotional or mental state, although it is subjective, qualitative, and not quantifiable by the methods of physical science? Bateson was intrigued by such direct knowing when he encountered it; he would have answered the question in the affirmative. Both McCulloch and Kubie had considerable empathy for others, but they excluded empathy from their discussions—probably because of the scientific ethos dominating the conferences.

The diversity of attitudes toward psychoanalysis among the conferees reflected not only its questionable legitimacy and the rivalry between psychoanalysts and neuropsychiatrists but also the personal experience of each participant. The very heatedness and irrationality of the discussion following some of Kubie's presentations attests to strongly held biases. Kubie's enthusiasm doubtless reflected his own favorable experience as analysand and analyst. While probably only few of the participants had been psychoanalyzed, many had first-hand acquaintance with psychotherapies akin to but less rigorous than psychoanalysis. Everyone would have had at least one friend or acquaintance who had been analyzed, and everyone would have had an opinion about whether the process had been beneficial or detrimental.

Warren McCulloch had an extensive acquaintance with the field from his two years as a practicing psychiatrist at Rockland State Hospital, his work with patients during his internship at Bellevue, and his experience as an administrator of psychiatric research. Whether as a young man he had undergone psychoanalysis, for example, as part of his training, is not known. At the time of the conferences he objected to psychoanalysis on various grounds. He knew of the evidence that psychoanalytic treatment of psychotic patients, even of neurotic patients, led to recovery with no greater frequency than no treatment at all. Thus for him the therapeutic claims of psychoanalysis were a cruel deception. Another of his objections was that the process of psychoanalysis severely violated the dignity of the individual and reduced his talent and individuality to symptoms, interfering with unique styles of thought and invention. A third objection was to the emphasis on irrationality, a fourth to the slipperiness of psychoanalytic hypotheses, which made scientific tests of validity impossible. Thus McCulloch and Kubie clashed not only on their views of psychoanalysis—its efficacy as well as its scientific status—but also on their views concerning the significance of inventiveness or creativity in the scheme of things. McCulloch came out of the closet in his antipsychoanalytic views in a talk he gave at the Chicago Literary Club, where he gave free reign to a style of presentation that was simultaneously literary, substantive, extravagant, and angry.[42] Freud had betrayed scientific (i.e., neurobiological) psychiatry. He would bury Freud, who had written *The Future of an Illusion,* for McCulloch titled his talk "The Past of a Delusion," and lamented that "unfortunately Delusions are Ideas. We cannot bury them properly. Only the dust of ages can consign them to oblivion." He judged that "the Freudian scheme is a tissue of unverified and often unverifiable hypotheses, all oversimplified." Indeed, psychoanalytic thought is a conceptual scheme and a method rather than a scientific hypothesis subject to clear experimental test.[43]

With the help of cybernetic notions McCulloch impugned much of social science, especially psychoanalysis, on the grounds that the data itself is distorted if not created by the theory:

> Interpretations of chaotic dreams are still controlled by theory, and that theory was in the head of Freud. Change this, and you have changed the method and the data. This is the curse of all attempts to understand things social. . . . What we seek to understand is coupled back through us, so that we ourselves change the thing we seek to understand. . . . When this coupling grows very close it is, in Freudian lingo called 'transference.' Freud himself came to attribute to transference what therapeutic value lay in analysis.[44]

Notably, McCulloch did not at this point dispute that transference may have therapeutic value but only that even if it works, that does not prove the scientific validity of the theory.

It may be that the effort to make the case for psychoanalysis on its merits as a scientific theory sells it short, and that its best contribution toward elucidating human life is of a different kind. As McCulloch knew, it is precisely the phenomenon of transference that needs to be appraised. As Michel Foucault pointed out, psychoanalysis directly invites motives and desires, however hidden, and human finitude—factors implicitly underlying the creation of all science, particularly social science—into the realm of knowledge.[45] It invites unreason as well as reason. It provides a critique of what makes the human sciences possible, and that is its strength, but as science itself it has severe limitations. Foucault observed:

> It [psychoanalysis] cannot span the entire field of representation (of the human), attempt to evade its frontiers, or point towards what is more fundamental, in the form of an empirical science constructed on the basis of careful observation. . . . All analytic knowledge is invincibly linked with a praxis, with that strangulation produced by the relation between two individuals, one of whom is listening to the other's language, thus freeing his desire from the object it has lost (making him understand he has lost it), liberating him from the ever-repeated proximity of death (making him understand that one day he will die). This is why nothing is more alien to psychoanalysis than anything resembling a general theory of man or an anthropology.[46]

The innovation of psychoanalysis, then, was to challenge the human sciences by introducing a different way of knowing. It called attention to a kind of nonscientific experiential knowledge that it is difficult to label and place within the total realm of knowledge—what Foucault calls the "domain of the modern *episteme*" of the human sciences.

Another level on which McCulloch attacked psychoanalytic practice was in the realm of money and the politics of the profession: He pointed out that in Chicago psychoanalysis so controls the teaching hospitals

> that no one may be a resident in psychiatry unless he is approved by them for membership in their sect. To become a member he must be psychoanalyzed, for which the analysand must pay the analyst who took the Hippocratic oath. . . . Some neophytes submit . . . for a share in the loot. Some, after conversion, believe they have something to sell. Convenient for them! It is still to their profit, or power which is profit, that this sect and its fellow travellers have oversold psychiatry. When I see such a sect prospering I do well to be angry.[47]

Elsewhere he says that Freud's followers, "the latter day illuminati, those new perfectibilians, have dethroned reason but to install social agencies, analytic interviews and transference in the place of espionage, confession and conversion."[48] Decades later Foucault also pointed out the political-power aspect of psychoanalysis and its kinship to espionage and confession, especially in its insistence on the therapeutic value of a patient's detailing his or her sex life to a therapist.[49]

McCulloch objected as well to how psychoanalytic practice handled ethics and morals, which were understood as the "material consequence of the way your parents inevitably handled you," a variant of determinism that avoids taking responsibility for one's choices.[50] He steered troubled people away from psychotherapists. He was annoyed that one cannot have an honest argument with a psychoanalyst, who is likely to respond by attacking your hidden motives rather than the logic of an argument: "Delusions defend themselves that way," he said. In 1952, when Marxists were much maligned and harassed in the United States, McCulloch drew an analogy between Marxists and Freudians in terms of the rigidity and deterministic character of their beliefs. He clearly disliked both, but included the true believers following Mohammed or John Knox in his criticism:

> Ruthlessly: Relentlessly: Remorselessly: They force their creeds upon us. For them life never is the game we play to our lives' end with fate and fellow man for keeps, but chiefly for the fun of it. How come these men to lack the humor and humility that keep us human? What strange defect to think one knows God's will, or Matter's dialectical determination, or how his brain works to fool him![51]

For Warren McCulloch "the game we play to our lives' end with fate and fellow man for keeps, but chiefly for the fun of it," often had to do with love and sex. Most psychoanalysts, particularly Kubie, had strict ideas about what was sexually mature and acceptable; McCulloch believed in an unrestricted sex life. His was what a later generation labeled an "open" marriage. His life style was, in that day, an outrage to the morality espoused by the New York Psychoanalytic Institute.

If McCulloch totally rejected psychoanalysis, he believed in an alternate approach to mental suffering. "There is one answer, only one, toward which I've groped for thirty years; to find out how brains work." He mentions in his Chicago lecture cretinism, paresis (the psychosis resulting from an advanced stage of syphilis), diabetes, all of which have been overcome with the help of biochemicals. He speaks of some of his collaborators, "youngsters who have learned respect for the physics and chemistry of living brains. Through them, and men like them, we may expect in time to cure—or better yet, prevent—psychoses." McCulloch was as convinced of the promise of physiological and logical approaches as Kubie was of psychoanalysis.

When McCulloch was working as a psychiatrist at Rockland State Hospital (1932–1933), he learned from his colleague Eilhard von Domarus to analyze the language and grammar of psychotic patients for structural peculiarities and disturbances of logic, which were different for different types of psychosis, but could be interpreted as reflections of disorders of thought.[52] (Bateson later also become interested in the analysis of the language of psychotic patients.) This was a logical but not a physiological approach. McCulloch closed his 1948 letter inviting von Domarus to the fifth conference with "Yours for scientific psychiatry."[53] Von Domarus's magnum opus, "The Logical Structure of Mind," was a doctoral dissertation under philosopher Filmer Northrop, McCulloch's friend and teacher. The dissertation is a work of philosophy of science concerned with a proper way to unify "the science of alienation from society (i.e., psychosis) and the study of physiological psychology." McCulloch found "that no other text so clearly sets forth the notions needed for an understanding of psychology, psychiatry and finite automata."[54] McCulloch thought von Domarus's work (which was completed in 1934) was on the right track philosophically. In his own formulation, at the time of the cybernetics conferences, McCulloch saw the science of signals and messages as the bridge between "psychology and physiology in the understanding of diseases called 'mental.'" Messages and signals have a material form, yet can be true or false and contain ideas.[55] Thus the unnatural Cartesian dualism (mind and matter) could be transcended. The ultimate theory of madness, its cause, characterization, and cure, would then be formulated in terms of nervous nets constituting automata and in terms of communication to, from, and within such nets.

When in 1941 McCulloch became the head of the research laboratory of the Neuropsychiatric Institute of the University of Illinois Medical School, he saw its mission as laying the biological foundation for a scientific approach to madness. He had not yet met Walter Pitts. Aside from some government grants for studies on chemical warfare during the war years, the work was supported by two large research grants, one from the Macy Foundation, for which McCulloch was answerable to Frank Fremont-Smith, and one from the Rockefeller Foundation.[56] In a 1949 review of the physiological processes underlying neurosis, he reported on work done in his and others' laboratories: one neurosis he had studied in detail, and on which he had collaborated with psychoanalyst Franz Alexander, started with "a group of erstwhile energetic people whose living is crystallized about some goal made unattainable by Fate. Thereafter they do their daily chores without zest. They are always tired . . . ," a condition named "vegetative retreat," or simply loss of zest.[57] McCulloch studied the rate at which sugar injected into a vein disappears from the bloodstream and found it to be unusually high for those in vegetative retreat. When the effect of exciting games and movies on sugar metabolism was studied, the results were inconclusive.[58] No "cure" came out of that particular study, only some information on sugar metabolism.

Another category of mental disorder McCulloch studied was the common war neurosis popularly known as "shell-shock": "A man near an explosion, but not so near as to suffer organic damage to his brain, starts to run in a frenzy. After being stopped, say long enough to smoke a cigarette, he has lost all memory of the events from just before the blast until he smoked the cigarette. Thereafter he is startled easily, is terrified by the sound of airplanes or the back-fire of a passing car, and dreams of battle frighten him. He comes to resemble a hound gone gun-shy."[59] McCulloch mentioned that after the Civil War "the patients were made drunk, and, as they sobered, were allowed to relive and discuss the things that racked them most." McCulloch favored amytal followed by coramine, for H. D. Fabing had found that most of his patients "lost their terrors and their amnesia without analysis, synthesis or intentional suggestion by any fellow man . . ."[60]—a genuine chemical cure, McCulloch happily announced.

He also extolled the British drug myanesin, as making manageable the withdrawal from alcohol by an alcoholic, from morphine by an addict, and even the loss of a sexual partner.

McCulloch had supported studies of the precise neurophysiological consequences and mechanisms of electric shock, chemical convulsants, and neurosurgery, all thought to have therapeutic effects,[61] but he was wary of and at times actively opposed the increasingly popular radical surgical procedures, "for until we know more than we do today . . . we are in danger of robbing the patient of some of the ends of life which make it in the long run socially worth while."[62]

McCulloch chose to consider as a prototype a condition known as "causalgia": this consists of a severe burning pain in a wound, so sensitive that a mere breath of air results in pain. The mechanism can be described in considerable physiological detail in terms of nervous paths and feedback loops that the nerve injury characteristic of causalgia disrupts.[63] This ailment is of identifiable organic origin, and various types of physical cures are known. Although his evidence is meager, McCulloch suggests that all the psychoneuroses and psychoses might some day be similarly analyzed, understood, and cured. The reliance on the physiological to affect psychological well-being and the willingness to tamper with people physiologically, within limits, were part of his outlook.

Kubie also believed in a biological substratum of neurotic and psychotic patterns, as for that matter did Freud. But to Kubie's mind neurophysiology was still too primitive to rely on for therapies. Kubie regarded psychoanalysis as by far the best available therapy for neuroses, though he would be hard put if asked to show in more than anecdotal fashion the efficacy of the method.

McCulloch's blast at psychoanalysis, "The Past of a Delusion," copies of which he sent to his friends, was received with applause by many of them, for it articulated their views. Karl Lashley, perhaps the most outstanding neuropsychologist of his time and director of the Yerkes Laboratory of Primate Biology managed jointly by Yale and Harvard, replied:

> Thanks for the most delightful Christmas present that I have received for many a year. . . . I would like 500 copies to circulate among my analytically inclined friends and enemies. It will not make you popular in Boston, but what the hell. I lost my anti-analytic fight there, but came out of it well, with a research professorship, an increased budget, and complete freedom from all academic responsibility.
>
> Have you seen Eyseneck on the value of psychotherapy (J. Consulting Psychology, 1952, p. 319)? Improvement, under analysis, 44%; other therapy, 64%; untreated, 72%. Sampling method invalid, but the best available.[64]

Heinrich Klüver wrote with apparent glee, "The whole thing looks very nice to put it mildly: the green cover contrasting with the red-hot emotions that the content is going to arouse (since even our, I thought gentlemanly, treatment of Kubie at the Cybernetics meetings can arouse such passions . . .)."[65]

At the sixth conference Klüver had suggested a new direction for research: studying structure of situations that produce childhood trauma (fundamental in Freudian theory) so as to identify how they differ from situations producing nontraumatic childhoods. From McCulloch's young friend in England, Turner McLardy, who had been a guest at one of the cybernetics meetings, came a note, "just to exclaim my joy and delight (and marvel, as ever) at your masterly, inimicable parting broadside to Chicagoan persecutors via the Literary Club!"[66]

Kubie learned of McCulloch's talk before the Chicago Literary Club from an alarmed member of the audience:

> Warren McCulloch, whom I regard as a good friend of mine, has recently read a paper. . . . The paper was a vitriolic attack on 'Freudianism,' on psychoanalysis and psychoanalysts. During the discussion which followed . . . he quoted you as saying "that if you helped one of ten patients you were satisfied, and if you cured one out of twenty you were happy." This does not sound like anything of yours. . . . Nevertheless, it carried a great deal of weight in the discussion, particularly since McCulloch described you as one of the few analysts that could be trusted. Would you be willing to affirm or deny this quotation from you?[67]

Kubie replied within a few days.

> Warren McCulloch, whom I also look upon as a good friend, is also an old devil; which you can tell him from me. In the old days, although he has always been antipathetic to analysis and skeptical of it, he was never vitriolic about it. That was one of the reasons why he and I have had many a friendly, kidding, scientific exchange in our effort to find common ground between his physiological approach and the analytic approach. . . . The more recent vitriole may be due to an accumulation of personal frustrations of his own displaced onto analysis. . . . It certainly can do a great deal of harm, and when I see him at a Macy conference in March, I will do what I can to quiet him. . . .
>
> As to his alleged quotation from me, I do not recognize it or anything remotely like it. . . . Although I have many reservations about therapeutic efficacy, and although I believe that many basic technical problems of analytic therapy are still unsolved, I cannot remember any moment in which my therapeutic pessimism descended to such depths as would give rise to the statement which Warren attributes to me. . . . What I have said and written is essentially (1) that we do not know our percentage of success, whether partial or complete; and (2) that this is not due to indifference on our part or to reluctance to subject our results to statistical analysis, but to the fact that the valuation of results in our field presents exceptional difficulties.[68]

McCulloch, for whom intellectual controversy could not destroy friendship, wrote to Kubie primarily about other topics but mentioned the paper:

> I take it . . . that some rumors of a paper of mine on "The Past of a Delusion" before the Chicago Literary Club have reached you. During my absence in New Orleans for Fat Tuesday a piratical version of that paper was produced. If I can highjack a copy for you I will. Until you have read it please take no stock of rumors.[69]

In the same letter McCulloch enclosed a scientific article and mentioned he would like to discuss its content with Kubie in some detail, "if we can sneak out for a cocktail hour or for a walk during the Macy meeting." Later, some time after having read "The Past of a Delusion," Kubie recalled, in a letter to McLean,

> I wrote a 10-page blast at Warren when I first saw that paper . . . and have been keeping it back. . . . In our consideration of this paper we should keep in mind that in all probability he was going through a disturbed episode. One can sense that in the paper itself. Of course that does not make the paper any the less harmful.[70]

About a year after his Chicago Literary Club lecture—he had by then moved from Chicago to MIT—McCulloch gave a talk on electrical potentials in the nervous system to the Neurological Study Unit at Yale University. Something about his talk or behavior was erratic and caused criticism. His host at Yale, John Fulton, although himself concerned, quickly came to his defense:

> We all know Warren of old. He is obviously a man of genius, and if at times he may seem slightly unstable, this very outgoingness is a part of his personality, and we all admire his absolute honesty and the loyalty he inspires among his junior colleagues.[71]

But Kubie, inclined to see others' demeanor in terms of psychological illness, understood the reports of McCulloch's behavior to mean that he was in a state of "catatonic excitement."[72] He wrote to Fulton (with a copy to Fremont-Smith): "I am distressed by this news about Warren . . . in him the boundary between sickness and health has always been narrow."[73] From Kubie's perspective McCulloch needed a psychiatrist's or psychoanalyst's help, and since McCulloch would not seek it, Kubie offered to phone a couple of colleagues in the Boston area who might take the initiative, "even on a social pretext if necessary." McCulloch, of course, would have been outraged, had he known of Kubie's kind intentions of "helping" him.

Then Kubie mentioned a circumstance, albeit in the language of psychopathology, that was destined to have an effect on the history of cybernetics:

> One force which may be relevant to his upset is the fantastic state of megalomanic and paranoid rage in which Norbert Wiener seems to have been ever since Warren came to MIT. . . . I could easily imagine that after their long and close association, Wiener's paranoid rage against Warren might touch off a like state in Warren. It is tragic and makes me feel heartsick; the more so because when we face this particular type of illness we are so helpless.[74]

Norbert Wiener's anger at McCulloch, which Kubie happened to observe, was a fact. Mrs. Wiener too was strong and never relented in her anger at McCulloch. The blow-up arose in part from the two men's differing temperaments, outlooks, and life styles, but was the specific result of some personal matters involving McCulloch and members of Wiener's family. All possibility of collaboration at MIT between McCulloch's group (which included Pitts) and Norbert Wiener ended forever, although earlier all had eagerly anticipated such cooperation.

To gain perspective on the differences in outlook between Kubie and McCulloch—whose friendship survived such differences and may have thrived on them[75]—I asked Macy participant Molly Harrower about them.[76] She knew both men professionally and personally. Born in Johannesburg, South Africa, Harrower had begun her education in England, and been assistant to C. K. Ogden (who developed Basic English) at Cambridge University for a time and then, at his suggestion, came to America to work with Kurt Koffka, one of the founders of Gestalt psychology. As a graduate student she had performed an extensive series of experiments with Koffka on the interplay of color, brightness, and form in perception; the theme running through the experiments is that these are nonseparable aspects of a single event of "field organization." She was the only student at Smith to complete a Ph.D. (1934) under Koffka. She found the Gestalt approach to psychology far more congenial than the behaviorists', then prominent in the United States, and it informed all her subsequent work in psychology.

For five years, under Rockefeller Foundation auspices, Harrower worked with Wilder Penfield's experiments in Montreal. Penfield was stimulating electrically parts of the cortex of patients during brain surgery, thereby often evoking specific sounds, memories, visions, or dreams. Harrower, as psychologist, was asked to observe actions and listen to the voluntary and involuntary words of the patient. As Harrower recalled during our interview, "It was very wearing, because I was concerned with the person as a person and my scientific interest gave way when the person was distressed. I was concerned with coping with the patient's anxiety." Anxiety often resulted when a patient, upon stimulation, suddenly said words that he or she did not want to say. Penfield's famous empirical studies strongly suggested precise localization of memories, "flashbacks," within the temporal lobes, and thus gave new form and content to Franz Gall's early ideas of brain localization. After completing her tenure in Montreal, Harrower began work in extending the usefulness of the inkblots known as the Rohrschach test. She served as a Macy Foundation fellow and a consultant to the Surgeon-General, as well as to the State Department, during the war years, using projective tests such as the Rohrschach to screen people for the military. In such testing, as in her later work as a clinical psychologist, Harrower emphasized meaningful wholes and patterns in the spirit of her Gestalt point of view.

She had met Fremont-Smith in 1936, Larry Frank soon thereafter, Kubie in 1943, and had known McCulloch for years before the cybernetics conferences. At the first meeting in March 1946 she had described the stereotyped and impoverished character of the responses to an inkblot test typical of individuals with organic brain damage and contrasted them to the variegated, rich responses of people with intact brains.

Harrower and McCulloch shared a strong interest in poetry.[77] Kubie and Harrower were friends and had shared responsibility for a 1947 Macy conference on the training of clinical psychologists.[78] These interlocking friendships demonstrate again the ingrown quality of the relationships among participants selected for Macy conferences. Elaborating on how McCulloch's and Kubie's attitudes to psychoanalysis differed, Harrower said:

> It was not only an intellectual difference, it sprung from something very near to the core of each individual . . . Warren was absolutely convinced that it was a basic indignity to a human being to be subjected to that kind of thing. His whole philosophy of life was absolutely poles apart from the analytic procedure. He was such an arch-individualist, feeling that everybody should be free to follow their inward dictates. The idea that somebody should sit outside and sort of pry into you was really horrible to him. He would never even consider any such therapy. He thought that he could surmount everything, and he probably did in his own way. . . . Kubie thought that his life had been revolutionized by the experience of psychoanalysis. Warren stood for independence from this kind of supposed help which wasn't help. And also, the things which analysis would consider pathological, Warren would endorse and claim were the best things that sometimes humans can do. Kubie felt just as strongly. Like a profoundly religious person believes in the Bible, he believed that there was a right way of doing things, and this was the way shown by psychoanalysis. Both he and Warren represented their truth by the way they lived. Warren resented any tampering with the personal development of the individual, which as far as he was concerned should proceed as the individual wants. What exactly he did with people who were distressed from their neurosis, I am never quite sure. . . . Did Walter Pitts commit suicide? From Kubie's point of view, Warren prevents somebody like that from getting the help they need. And Wiener was in a way pretty naïve. Of his nuttiness Warren would say: This is an integral part of his creativity.[79]

Kubie was interested in Pitts at the conferences: "I tried to befriend him, I liked him. But it was not possible. . . . A very sick person."[80] Harrower's comparative description of Kubie and McCulloch is congruent with my other knowledge of them.

McCulloch admired creative thinkers and scientists and fostered the scientific development of younger colleagues. But how does one tap one's own and encourage others' capacity for scientific invention, philosophical depth, and production of tangible results? McCulloch seemed to need collaboration with others. His most innovative work was carried out in collaboration with Walter Pitts. To encourage others, he might help them out when they were financially pressed or offer the hospitality of his home, loving personal friendship, and stimulating conversations.

Kubie was deeply interested in psychiatric aspects of creativity, in artists and writers as much as in scientists. At the meetings and elsewhere he presented psychoanalysis as a science, but he apparently also viewed it as in competition with and superior to the arts. In his most widely read book, *Neurotic Distortions of the Creative Process,* he described instances of transference and symbolic statements in psychoanalytic treatment and concluded each description with such comments, as, “What work of art can achieve a condensation greater than this?,” or “Can any art form do more?”[81] In his essays he borrowed metaphors from cybernetics: he compares the preconscious to an electronic computer and free association to a scanning operation, and he spoke of informational bits. Kubie’s dichotomy is Manichean; the “neurotic” process is “sick” and the “creative process” is “healthy,” but “these intertwined but mortal enemies, the creative and the neurotic processes, are universal.”[82] Psychoanalysis frees the creative process, according to Kubie. Resurrecting the “preconscious” originally posited (and later dropped) by Freud, Kubie gave a description of “the process”[83] of creative work that many artists and writers recognized as valid.[84] Kubie believed that the arts “automatically provide individuals with ways of hiding their conflicts while at the same time giving them partial gratifications. In this sense they reward the neurosis in us until we become psychotically disorganized, or commit suicide.”[85] Among these conflicts Kubie particularly singled out the “drive to be both sexes.” Kubie did not seem to think highly of the cultural and intrinsic value of the products of artistic and literary creation: “I have no belief that in and of themselves the arts automatically exercise a creative influence in human life,” he wrote. “They have added little to the sum total of human wisdom for the creator or for the spectator or auditor. Like so many other products of the human spirit they have let humanity down badly.”[86]

Yet the impression one gets from his writings is that Kubie admired and sought the very freedom symbolized by the phrase “preconscious activity.” He was attracted to literary people. Playwrights Tennessee Williams and Moss Hart and writer Charles Jackson (*Lost Weekend*) were among his analysands. Kubie tried unsuccessfully to change Tennessee Williams’s sexual orientation; according to Williams, Kubie did help him to stop hating his father.[87] Kubie reviewed work by Arthur Miller (“This article is written out of the author’s unhappy conviction that art, music and literature have failed humanity.”[88]), William Faulkner,[89] Ernest Hemingway, and others. In the Hemingway review Kubie discussed Hemingway’s personal fears and bravado. The review was in galleys and about to be published in the *Saturday Review of Literature*, when it was stopped by a handwritten note from Hemingway: "I have turned over your letter and article to my attorney to take whatever action he sees fit. You will have to wait until I am dead, I'm afraid, to libel me with impunity."[90] Kubie's articles in the *Saturday Review* were preceded by an editor's comment, prepared in consultation with him, introducing

> Dr. Kubie's series of articles applying the principles of psychoanalysis to the modern literature of neuroticism . . . these essays are not literary criticisms but *scientific* analyses . . .[91]

Not all psychoanalysts agreed with Kubie. Otto Rank, for example, saw much that is constructive, courageous, and hopeful in artists' struggles, however neurotic, contrasting them with the majority who settle for uncreative conformity.[92] Nor did Harrower agree with Kubie—in fact, she espoused the therapeutic value of writing poetry.

In his day Kubie was a leading authority on the creative process, although he was simultaneously its admirer and its detractor, and considered art and psychoanalysis to be competitors. This conflict seems personal rather than dictated by traditional psychoanalytic theory. Kubie was conscientious in his self-analysis, which he regarded part of his responsibility as a practitioner, and spoke readily of his "unconscious conflicts over sexual identification and sibling rivalry,"[93] especially in connection with his choice of profession. He later elaborated: "My going into medicine was related to the death of my mother, when I was a small boy. When she died I immediately stopped eating. Consequently I am shorter than most of my family, not so much total body size, but in particular leg-length. I of course then looked for competitive activities where my legs didn't count."[94]

Kubie struggled longest and hardest with what he called "the drive to become both sexes," an extension of the Freudian notions of penis envy and castration fear, which in Kubie's formulation was coupled to his view of creativity. He thought about it in the 1930s, presented a first version of his paper to the American Psychoanalytic Association in 1954, but wasn't content with it, and finally prepared it for publication before his death with the words, "It has been rewritten many times. . . .

Now the process of postponement must come to an end."[95] He acknowledged that his thesis "brings me into conflict with artists and indeed with almost all creative people in the world, although more in the arts and letters than in the sciences." He pegged his discussion on Virginia Woolf's *Orlando,* which he characterized as "a story written by a woman about a man who turns into a woman and then back and forth between the two, but without ever losing completely his hold on maleness . . . a book of moving beauty and sadness." He attributed Virginia Woolf's suicide to her having succumbed to the drive to become both sexes, which results in efforts to "achieve mutually irreconcilable and consequently unattainable identities." "Was there ever a clearer or more tragic demonstration of the fact that the creative process can be used as a defense against therapeutic insight?" he asked. He went on to interpret many kinds of behavior (indecision about career, sadness after sexual intercourse, insatiable appetite for food, money, or sex, apparent androgyny of members of the 1960s hippie culture, and so on) in terms of the drive to become both sexes, and gave an assessment of its importance:

> I will not claim that this drive occupies an exclusively central role in the psychodynamics of all psychopathology. I can only say that I will not be surprised if in the end its importance should prove to approximate such a central and primary position.[96]

Kubie found Margaret Mead disturbing and irritating because she seemed to encompass the masculine and feminine within her person to an extraordinary degree.[97] Of course her professional views of male and female, and anthropological research on the subject, were well known.[98]

Although Kubie's book struck a responsive chord for some artists and writers, others dismissed him on the grounds that his perspective was distorted and he was generalizing to the whole culture from the peculiar sample of humanity he encountered in his psychiatric practice. One writer playfully suggested changing the book's title to read "creative distortions of the neurotic process"—a title that more closely reflects what many artists are actually about.[99] Kubie's analysis of Virginia Woolf's troubles contrasts sharply with more recent studies, which attribute much of her difficulties to sexual abuse during her childhood.[100] A persistent question, not laid to rest by Kubie, was whether psychoanalytic treatment would destroy the creative and original in favor of adaptation.[101]

A view nearly diametrically opposed to Kubie's of the link between the creative individual and insanity is offered by Foucault, whose history of the relation of madness to Western civilization offers a cultural and philosophical appraisal.[102] He speaks of van Gogh, Nietzsche, Swift, Artaud, Rousseau, and others. He sees the creation of a work of art and its truth as the antithesis of madness. Since they explore beyond the commonplace, madness and art are in competition. It is a misunderstanding to attempt to reduce art to neurosis or psychosis. "Artaud's madness does not slip through the fissures of the work of art; his madness is precisely the *absence of the work of art*. . . . Van Gogh knew quite well that his work and his madness were incompatible. Madness is the absolute break with the work of art . . . it is the very annihilation of the work of art, the point where it becomes impossible and must fall silent."[103]

Harrower responded to Kubie's 1954 version of the drive to become both sexes with a long letter to Kubie. Her detailed critique of his paper is largely couched in the form of relaying the critique of two of her friends who had also researched the subject:

> I think they tend to see the drive to be both sexes not as you have somewhat emphasized in the article as a drive which may bring disturbances in its wake, but rather for them its explicit recognition constitutes one of the most powerful, health-giving factors and results in the richest interpersonal experience including sexual experiences, when shared by marital partners . . .[104]

Judging from the final version of Kubie's essay, however, he did not budge from his original views.

Since 1955, when Harrower wrote to Kubie, society has adapted to the drive to become both sexes by radically changing patterns of relationship between men and women and of social function to gender. These changes favor Harrower's optimism about the drive to be both sexes rather than Kubie's pessimism, although it would be frivolous to assume that the darker, destructive side noted by Kubie has automatically evaporated.

The respective theoretical positions regarding psychiatry of McCulloch and Kubie were clear, sharp and distinct. They serve to provide a good polemic highlighting the points at issue.

Each defended his position strongly. Harrower was more pragmatic and more generous in encompassing the diversity of views. At the time of this writing (1988) advocates of the neurophysiological and psychotherapeutic schools of thought continue to perpetuate variations of the McCulloch-Kubie controversy, although it has become common to combine talk-therapies with the use of drugs. Indeed, since the days of the cybernetics conferences, progress in the study of the biochemistry of synaptic transmission has led to the development of drugs that have been partially effective in the treatment of schizophrenia, and other drugs that counter depression. Though these drugs generate diverse psychological changes of their own, their partial successes help to justify McCulloch's faith in the therapeutic promise of studying the chemistry of the brain.[105] But experience with drugs has pointed up the subtlety of the interplay of mind and brain and the relative crudeness—and hence unsatisfactoriness—with which drugs deal with it. Great confidence was unjustified for either school of thought. We have called attention to a difference in their outlooks on life. In fact their controversy was primarily over philosophical first principles and political values, although the context was psychiatric practice. Incidentally, McCulloch has described himself as voting Republican, and Kubie has described himself as voting liberal-Democratic.

Both Kubie and McCulloch focused on the individual when discussing mental health or illness. Kubie spoke of the unconscious, the preconscious, and the conscious, but not of political conditions or of marginality to the status quo. Wilhelm Reich, who had most brilliantly linked political conditions to psychoanalysis, had been ostracized by the psychoanalytic community. Larry Frank and Margaret Mead, like Erik Erikson, represented a view that emphasized the cultural, but none of them—in this era of social and political amnesia—pursued the causes of mental suffering sufficiently far to locate them primarily in political and economic conditions.[106]

This deepening of psychoanalytic theory was probably retarded by the hostility to radical thought, especially Marxist thought, that prevailed at the time of the conferences. If we forego the tendentious metaphors of mental "health" or "illness," we are led to considering psychiatry's premises about human life and the larger world. The west has long fostered individualism in many forms; McCulloch's and Kubie's variants on the theme both belong to that heritage.

Artists, intellectuals, and scientists are crucial to society, artists for providing a feedback that confronts society with itself, and scientists for providing new knowledge. Kubie seemed to be willing to forego artists and creative writers because he saw their individual struggles as self-defeating. And perhaps individualism itself, together with the patterns of a mass-society, militates against an interactive, communitarian outlook that values responsiveness, vulnerability, and sensitivity to others—an outlook that depends on a high level of feedback and that may have much to offer society.[107]

Bateson and the Cybernetics of Deranged Minds

Notwithstanding the disagreement between Kubie and McCulloch, the cybernetics conferences made a unique contribution to the theoretical formulation of problems of insanity and toward a reconciliation of psychoanalytic and neurophysiological views. At the first conference, in March 1946, Wiener criticized the Freudian notion of libido as inappropriate and argued that "information" is a more suitable basic concept for describing psychological events. McCulloch echoed and elaborated this idea when he spoke of messages and signs as the psychobiological elements, which have a physical aspect and may at the same time carry significance and be true or false. Kubie criticized the Freudian use of the libido concept as fallacious and, in particular, found the quasi-quantitative economic principle for libido unjustified.[108]

Gregory Bateson approached this problem from a fresh angle, perhaps a bit naïvely—but he had the advantage of coming from neither the psychoanalytic nor the neurophysiological tradition. His experience in evolutionary biology, social anthropology, and learning theory had made him an acute naturalistic observer of social activities, and he liked theoretical construction. He was accustomed to recognizing the role of the social and cultural, and that led him to ask questions that Kubie and McCulloch didn't think to ask. Bateson was by temperament disinclined to criticize other researchers and preferred to focus on his own constructions. He gladly remained outside of the acrimonious internecine rivalries within psychiatry. (An associate Bateson had trained, Jay Haley, did openly take issue—with a humor akin to Bateson's—with conventional psychoanalytic and psychotherapeutic practice.[109]) Bateson resembled McCulloch in that his interest in the pathological was only incidental to his interest in the human creature generally.

At the time of the conferences Bateson was for the first time undergoing a quasi-psychoanalytic psychotherapy.[110] After a visiting professorship at Harvard, which was not renewed (Carleton Coon, an anthropologist, was chairman of the committee deciding on his reappointment[111]), Bateson moved to California to make a new beginning. He abandoned the study of non-Western cultures and accepted a position to study communication in psychiatry with the Swiss psychiatrist Jurgen Ruesch at San Francisco's Langley Porter Clinic. Like Norbert Wiener, Gregory Bateson had been after World War II much troubled by reflection on his wartime activities. His biographer speaks of Bateson's "revulsion at having participated in the crude manipulations of wartime applied anthropology," and suggests that his depression following work with the Office of Strategic Services in the Far East contributed to his impulse to seek psychotherapy.[112] Bateson's psychotherapist, Elizabeth Hellersberg, was a Jungian and became his personal friend. Kubie would not have considered it proper psychoanalysis, for it entailed a face-to-face relationship.

Emerging from wartime activities, Bateson was at somewhat of a loss as to where to turn next. But the conferences were providing him with new theoretical tools, and his new experience in psychotherapy, together with his work at the Langley Porter Clinic, provided this chronic participant-observer with new data. His new "fieldwork" was to observe the practice of psychiatry. As Ruesch wrote, he and Bateson

> . . . studied psychiatrists in non-controlled interviews in their homes, their offices, our offices, or wherever the opportunity presented itself. In this type of interview the focus of the investigation centered in the interaction with the psychiatrist, in order to gain a better picture of the informant's interpersonal approaches. In addition . . . about thirty different psychiatrists were recorded on wire with the knowledge of the participants. . . . We have recorded many hundreds of hours of therapeutic sessions. Several therapist-patient teams were followed longitudinally and many more teams were studied cross-sectionally. These recorded interviews were then analyzed by the authors for material pertinent to the value systems of both therapist and patient, and especially for the study of the modification of values in and during therapy.[113]

The U.S. Public Health Service had funded this empirical study.

How does a man like Bateson come to terms with a field new to him? He began by not only observing practices in the field but exploring rudimentary philosophical questions, a meta-psychiatry, which led him to a framework and perspective from which to view the practices. He started with codification, the concept he learned at the cybernetics conferences, which describes the processes by which people perceive and construct their knowledge of external events. By these processes "relations among external events are systematically translated into other relations among the events and processes of the mind."[114] Fundamentally, we think in terms of relationships, including our own relation to external events. Thinking in terms of things is a mere epiphenomenon, reinforced by the subject-predicate pattern of our language. Bateson recognized the existence of a rich psychic life, largely unconscious. Just as external events, what we consciously know of our own psychic life comes through a form of codification:

> Whatever may be the mechanistic or spiritual base of the phenomenon (of consciousness) it is certainly a special case of codification and reductive simplification of information about certain parts of the wider psychic life. . . . The presence of consciousness denotes an extraordinary complication of the psyche, and many specifically human problems and maladjustments arise from this mirroring of a part of the total psyche in the field of consciousness. . .[115]

Perception or codification is so intimately intertwined with a system of values, Bateson argued, that one should only speak of the single process, as codification-evaluation. Particular aspects of codification-evaluation include whether one perceives any specific communication as a report, as a suggestion for action, or as both; how one conceptualizes one's self, the boundary between self and the environment, and whether in particular circumstances one locates the center of control within oneself or the environment; the degree of abstraction one uses in codification; that abstract premises are often self-validating; and that internal contradications, i.e., ambivalence, arise in systems of codification-evaluation.

Bateson's hypothesis is that changes in a person's codification-evaluation process are an essential ingredient in proper psychotherapy, but that conscious insight need not accompany such changes. Such changes can also occur by other means, such as changes in body chemistry, or an organism's failure to achieve a goal, which alters some premises underlying its codification-evaluation, in effect, learning and correcting its errors. Bateson's abstract formulation encompasses and transcends both Kubie's psychoanalytic methods and McCulloch's biochemical/neurophysiological approach. Bateson's thinking, however, eventually took him in a direction different from either.

After these philosophical preliminaries Bateson came to his central theme, the communicative interactions between people. Our cognitive-evaluative processes are tied to such communications. Since communications can be seen or heard, studied, and analyzed, they provide an inviting subject of research for an anthropologist.

As Bateson wrote to Wiener,

> The data on which I work are the utterances and actions of mental patients and psychiatrists and myself. I collect sequences of communication which occurs [*sic*] in psychotherapy, and which sometimes generates [*sic*] changes in therapist or patient, or both. The changes in which I am interested are those intangible ones: changes in the premises governing an individual's actions and his apperception of the world around him.[116]

From the Macy meetings he learned that engineering theory was shifting its focus from energy to communication and information, and that Freudian notions of libido and energy appear as conceptually misleading in psychiatry, whereas messages and communication are appropriate if the ideas of cybernetics are valid. From his interviews of psychiatrists he had compared Freudians, Jungians, and the protest of the new "humanist" psychiatrists. He thought the Jungian attitude toward the unconscious preferable to Freud's dictum "where id is there ego shall be." In particular he favored the new approach exemplified by Harry Stack Sullivan, Kubie's *bête noir,* to the classic Freudian attitude:

> Sullivan's emphasis . . . on the phenomenon of interaction . . . is very clearly part of a defense of man against the older, more mechanistic thinking which saw him so heavily determined by his internal psychological structure that he could easily be manipulated by pressing the appropriate buttons—a doctrine which made the therapeutic interview into a one-way process, with the patient in a relatively passive role. The Sullivanian doctrine places the therapeutic interview on a

> human level, definite it as a significant meeting between two human beings. . . . The Sullivanian emphasis upon interaction is thus a metacommunicative statement of the value to be set upon man and upon human relations. It is a humanistic correction of older manipulative emphases.[117]

It was Bateson's understanding that the trend from physics to cybernetics converged with a shift from the Freudian to the interpersonal emphasis in psychiatry, and he conceptualized the latter in cybernetic language:

> If . . . we look at the same Sullivanian doctrine of interaction with the eyes of a mathematician or circuit engineer, we find it to be precisely the theory which emerges as appropriate when we proceed from the fact that the two-person system has circularity. From the formal, circularistic point of view no such interactive system can be totally determined by any of its parts: neither person can effectively manipulate the other. In fact, not only humanism but also rigorous communications theory leads to the same conclusion. . . .[118]

Norbert Wiener had used the language of communication theory to criticize social arrangements that favor one-way communication and inequalities. He described psychoanalysis in terms of "the concept that the stored information of the mind lies on many levels of accessibility and is much richer and more varied than that which is accessible by direct unaided introspection; that it is vitally conditioned by affective experiences which we cannot always uncover by such introspection, either because they were never made explicit in our adult language, or because they have been buried by a definite mechanism, affective though generally involuntary; and that the content of these stored experiences, as well as their affective tone, conditions much of our later activity in ways which may well be pathological."[119] Wiener thought that such a formulation, rather than the Freudian language, showed the compatibility of psychoanalysis with automaton and information-processing models of the brain. Making an analogy between memories in brains and information storage in complex circuits and computers, Wiener had written that shock treatments, although they may have deleterious effects too, are preferable to prefrontal lobotomies because they are at least a less drastic intervention.[120] His outrage at psychosurgery was conveyed when he commented that "prefrontal lobotomies have recently been having a certain vogue, probably not unconnected with the fact that it makes the custodial care of many patients easier. Let me remark in passing that killing them makes their custodial care still easier."[121] Bateson wrote:

> Among humanist psychiatrists, the assault upon the psyche which occurs in electric shock therapy, and in such operational procedures as lobotomy, is seen as gross and potentially destructive. The humanistic attitude toward these procedures may be summarized in a word: horror. But the horror which humanists express is no less than that expressed by the engineers who see in these operations a blind and stupid muddling, a destruction of the organism's precious negative entropy.[122]

The upshot of Bateson's two years of study of psychiatry and psychiatrists was the open-ended, yet anthropological view that "the theorist can only build his theories about what the practitioner was doing yesterday. Tomorrow the practitioner will be doing something different because of the theories."[123] He observed, however, that the theory of the *idée fixe* appears in one form or another in many schools of psychotherapy, and other theories can be translated into that form, so that the notion of an *idée fixe* may be the basis of a general theory of psychopathology. For a general theory of therapy, he confided to Wiener in 1952, he preferred the "unabashedly mystical" theory of a natural healing force or energy (*vis curatrix naturae*) and noted that a belief in curative forces is common among successful therapists.[124]

Within a few years Bateson moved way from a primary concern with psychiatry to a more general study of communications, but his contribution to psychiatric thinking had just begun. He is best known to psychiatrists for development of the double-bind concept in schizophrenia, and to counsellors for his cybernetic theory of alcoholism and for his and his coworkers' early attention to family patterns. The ideas Bateson encountered at the cybernetics conferences and through conversations with Norbert Wiener played a significant part in these innovations.

Before describing the circumstances and content of these influential ideas, I should like to mention two unpopular notions Bateson articulated later, in the 1960s. The first was particularly unpopular in scientific circles, while the second was unpopular in psychiatric circles.

Early in his study of communication patterns among schizophrenics, Bateson met the psychoanalytically trained psychiatrist John Rosen, who specialized in treating schizophrenics. Rosen had extraordinary success in rather quickly entering a patient's mental world and carrying out apparently successful treatment of so-called deteriorated schizophrenics of the catatonic, paranoid, and hebephrenic types, given up as hopeless by other psychiatrists. Bateson was intrigued with Rosen's unusual talent and, with his team, filmed, taped, and studied how Rosen worked.[125] Rosen's work confronted Bateson with examples of correctly knowing or spontaneously responding to another's psychological state, and with a level of communication that defied detailed analysis of observed behavior. Rosen's direct response to psychotics seemed to be a dramatic, powerful form of empathetic knowing, rather than knowing through scientific observation. Rosen described it as direct communication between the patient's unconscious and the therapist's unconscious.[126] Rosen found that psychotics who have been analyzed by him tend also to make good staff in caring for other psychotics. Somehow "the therapist's instinctual drives of love, hate and aggression must have come into such a balance, as he relates himself to the patient, that the patient will thrive. . . ."[127] Rosen required solid, direct interaction between patient and therapist, the antithesis of the antiseptic aloofness of Freudian psychoanalysis. Bateson was similarly impressed by Frieda Fromm-Reichman, a particularly gifted therapist working with schizophrenics. A decade later, when Bateson was working with dolphin communication, he was again in correspondence with McCulloch, who referred to "empathy" in a letter touching on cybernetics, theology, and personal philosophy.[128] In his reply Bateson wrote that empathy "is conventionally distrusted in psychological circles as an unreliable way of getting information. Personally I disagree with that distrust and regard empathy as essential for understanding either other men or animals."[129]

Bateson was thus implying that scientific techniques, in which observer and observed are clearly distinguished, are inadequate for understanding living creatures, and that the development of empathy is crucial as well. It is noteworthy that neither Bateson nor anyone else (certainly not Kubie) went sufficiently against the scientific grain of the cybernetics conferences to assert such an idea there. At the conferences Bateson did speak of "the body as a whole as a possible analogic calculating machine . . . able to contrive analogies with the observed actions of human beings with whom we communicate."[130] But Kubie's response to Wiener's insistence that good observation requires avoiding "resonances" was to explain how the psychoanalyst does everything humanly possible to be detached. Only Margaret Mead ventured that "it is a question of using resonances," not avoiding them.[131] Years later Bateson organized another conference, held in 1968 in Austria, to which he invited McCulloch. At the time McCulloch described himself as "an old man," although he was only sixty-nine. He was indeed frail, his hair and beard were white, his teeth were bad, and he drank too much. He was characterized at that time by conference secretary Catherine Bateson as "a curious blend of glee and grief, of belligerence and gentleness."[132] Empathy was under discussion, and McCulloch's comment is pertinent:

> I think you completely miss the flavor if you don't extend to inanimate objects, even when they're our own productions, our tools, if you don't want to call it sympathy, call it empathy. It hurts me when somebody picks up a sledging hammer or a blacksmith's hammer and uses it on a rock. I know what he's doing to the hammer. . . . But if you don't happen to have extended it, to feel as you would if you were it, then I say you've dehumanized the machine.[133]

Bateson's second unpopular stance was his disagreement with the conventional view that psychosis is necessarily an illness and that psychotics are to be placed in mental hospitals, with all that implies. He understood that such institutions favor schizophrenic behavior, because such behavior is well adapted to the social environment of most mental hospitals. Bateson had been studying schizophrenics and autobiographical accounts of ex-schizophrenics. He noted that spontaneous remission is regarded as a mystery in psychiatric circles, and it happens that an ex-schizophrenic is, to the puzzlement of medical people, "a better, happier and more imaginative man after his psychotic experience" than before. Both of these circumstances are to be expected if you look at it from Bateson's point of view:

> To evaluate a psychosis is perhaps impossible. Conventionally, schizophrenia is regarded as a disease, and, in terms of this hypothesis, both the conditions necessary for it and the precipitating causes which bring on the attack must be regarded as disastrous. . . . I have sug-

gested that the psychosis is more like some vast and painful initiatory ceremony conducted by the self. From this point of view, it is perhaps still reasonable to regard the conditional causes with horror. The precipitating causes can only be welcomed.[134]

The puzzle, in Bateson's view, is this:

> What needs to be explained is the failure of many who embark upon this voyage to return from it. Do these encounter circumstances either in family life or in institutional care so grossly maladaptive that even the richest and best organized hallucinatory experience cannot save them?[135]

Of course Bateson would have realized that both the medical model and his spontaneous-initiation-ceremony model are to a large extent self-validating and that the choice of model is enormously consequential. In the 1960s some psychiatrists began to work with schizophrenics using models along Batesonian lines.

Amid all the discussion of theory at the cybernetics conferences, the youngest regular participant was by the last few meetings in the process of psychological deterioration, a fact powerfully affecting all those who paid attention to it. It may be a commentary on the state of psychiatry at the time that Walter Pitts's deterioration took its course and, despite much good will, could not and would not be reversed. Nor did spontaneous remission take place. Information about this man with a penchant for anonymity is necessarily second-hand, but apparently his early home environment in Detroit had been extremely difficult. He found a new "home," however, as a teenager in Chicago and then at M.I.T., with McCulloch's family as well as with his close friends, where his peculiar talents were welcomed and appreciated. They admired and made use of his unequaled ability for understanding, retaining, critically appraising, and working out in detail the general ideas posed by McCulloch, Wiener, and others, in the light of an up-to-date knowledge of whole fields of inquiry. They also enjoyed his company. But after a few years Pitts distanced himself from his colleagues, lived alone, and all but ran away or seemed not to recognize anyone when he encountered one of them. He also stopped appearing for work at M.I.T. Harrower recalled: "Pitts was obviously very withdrawn, very dependent on Warren. He was quiet-spoken, almost inaudible, a young recluse, extremely shy. He probably felt extremely inadequate as a person. If I tested Pitts, I probably wouldn't dare even tell you what I found, because he'd probably be schizophrenic. . . . Did he commit suicide?"[136] He was judged "very sick" by Kubie, "schizophrenic" by Ralph Gerard and others—but what was he to do? Psychoanalysis would hardly have looked promising. He was interested in drugs and biochemicals, experimented with them, and even synthesized his own in his room at the Kirkland Hotel in Cambridge. Whatever benefits or detriments he found from the drugs, they did not seem to bring him back to a greater well-being. Although I have spoken to many people who knew him at Chicago, at the Macy, and at M.I.T., and have even received a report from his landlady at the Kirkland Hotel, I lack the detailed documentation for a full picture of his life.[137] He, who had been regarded by distinguished scientists as the greatest scientific genius of his generation, deserves a major biographical study.[138] The sad story of Walter Pitts—whatever the details—jolts one into remembering that the disagreements among Bateson, McCulloch, Kubie, and Harrower were not only academic ones but reflected a passionate concern for the quality of human lives. Yet one wonders whether the very intensity of McCulloch's relationship to that potent source of scientific knowledge and insight, the vulnerable Walter Pitts, did not in some way deflect Pitts's personal development, inhibit his autonomy, and help precipitate his decline.

Bateson's primary interest, however, was never pathology and therapy, but communications. Although increasingly research funds were coming from government agencies after the Second World War, Bateson relied first on the Rockefeller Foundation, which supported his project in California on paradox in communication, begun in 1952, and later (1954), when the Rockefeller did not renew, turned to Fremont-Smith and the Macy Foundation to support a more focused study on paradox in schizophrenic communication. The co-workers he assembled included one young man with literary interest and librarian's degree (Jay Haley), another who was trained as a chemical engineer and had been participating with Margaret Mead in studying Chinese culture at a distance (John Weakland), and a third with training in psychiatry (William Fry at first, but later Sullivan-trained Don Jackson)[139] Insofar as the project dealt with psychiatry, the emphasis was on observing and listening to the talk of schizophrenics and, especially, on seeing them interact with their families. As was his scientific style, Bateson coupled naturalistic observation at one end of the concrete-to-abstract continuum with abstract thinking at the other. At the Macy conferences he had talked with Norbert Wiener about Russellian paradox, a notion which Bateson had turned to use in connection with humor and other forms of communication. Bateson thought it could be used as well in the description of schizophrenics. To help clarify their ideas, Bateson, Haley, and Weakland secluded themselves in a cabin in the mountains for two days, where they argued out the formulation of what came to be known as the double-bind concept of schizophrenia.[140] Bateson drew ideas from many sources. In California he encountered his fellow expatriate Englishman, Alan Watts, a prolific author of books about Zen Buddhism. Bateson and Watts discussed at length the Zen counterpart of occidental psychotherapy, in which the Koan, a paradox to be resolved, plays a central role. Typically for Bateson, he was drawing his ideas from a mathematician on the one hand, and from a student of oriental mysticism on the other. Norbert Wiener was the sounding board for his ideas. As early as 1952 Bateson had suggested to Wiener that he suppose a computer

> were to suffer from a defect—say an *idée fixe,* a rooted memory or an erroneous over-specialization . . . is it not conceivable that to pose a paradox to the machines might be therapeutic? . . . All this leads to the possibility that the psychotherapist, dealing with a human patient might be able to improve his methods . . . he might be able to select that category of paradoxes which would in fact exercise [*sic*] the particular part which is stuck in the particular patient, always supposing that diagnosis would be good enough. But it also leads to a more difficult problem. Suppose the stuck part to be such that paradox is generated in the machine, even when non-paradoxical problems are presented, what sort of psychotherapy would you administer? (This actually seems to be a rather common type of pathology—and incidentally, is a pathology which might be generated by the type of therapy suggested above.)[141]

This letter reveals some of the questioning that lay behind the hypothesis given a definite form as the double-bind hypothesis four years later. What is remarkable is that the letter shows Bateson seeking clues for psychotherapy from Wiener's thoughts about computers. Bateson recalled that Wiener had once suggested to him that "a telephone exchange could be called 'schizophrenic' in a formal sense if it mistook numbers mentioned in the conversation between subscribers for those numbers which are the names of subscribers. The double-bind idea was born out of the question, 'how would one teach a telephone exchange to make this error?'"[142]

Once Bateson had formulated the double-bind idea, he immediately wrote to Wiener for confirmation of his own understanding of the logic involved. Others from the cybernetics conference, such as Henry Brosin and Alex Bavelas, would be consulted later in connection with the study. Some tried to test the double-bind hypothesis experimentally, but Bateson himself conceded a decade later that it is a "slippery" theory, not really a theory in the ordinary sense, but more like a new language or a new epistemology: "A language can be confusing or enlightening. It can be convenient or clumsy. But it cannot, in itself, be true or false."[143] The double-bind hypothesis provided an alternative to traditional psychiatric language for characterizing patterns typical in the etiology and behavior of schizophrenics. The new language centered on interactions between people (instead of, say, id, ego, and superego or the unconscious and conscious referring to one person's mind). It focused attention on patterns of communication and metacommunication in families in which one member was diagnosed as schizophrenic. In the 1950s Bateson and his associates brought schizophrenic patients together with their families to observe patterns of communication; this led Jackson, Haley, and Weakland—more so than Bateson—to experiment with using the double-bind concept for psychotherapy and to evolve therapeutic methods in which the whole family, not an individual, is treated, in what came to be known as conjoint family therapy. Much of their discussion of families used cybernetic models.[144]

Many of the ideas of Bateson and his group in California were applied in various ways by other psychotherapists. In particular R. D. Laing in England took up double-bind theory in treating schizophrenics, and the Palo Alto group's work was one of the streams feeding into the family therapy movement that grew in the 1960s. More recently family therapies have been criticized in that their systems-theoretic approach fails to acknowledge individual responsibility, for example in cases of child abuse.[145]

The Veterans' Administration Hospital in Menlo Park, California, where Bateson and his group worked, had not only schizophrenic patients but an especially large number of alcoholics. Again Bateson observed, but he did not publish a theory of alcoholism until 1971. Bateson knew that the Alcoholics Anonymous organization "has the only outstanding record of success in dealing with alcoholics."[146] His analysis of alcoholism as well as of the rules and structures of AA required critique of "the group of premises upon which Occidental concepts of the 'self' are built," and specifically an analysis of "the epistemologically unsound resolution, 'I will fight the bottle.'"[147] The analysis Bateson made was in terms of the paradox inherent in the notion of control of oneself and the resulting escalating positive feedback. Bateson titled his article on alcoholism "The Cybernetics of 'Self'". Alcoholic intoxication already provides a more correct state of mind than the conventional effort to "fight the bottle." The famous Twelve Steps of AA lead the individual to see him or herself as part of a system larger than the conscious ego, as is in fact true, thus serving to help correct an erroneous epistemology.

When some years later others filmed and observed the most effective psychotherapists in action and developed abstract, formal structures describing a new approach to psychotherapy in terms of interactions between therapist and client, Bateson congratulated them in a foreword to their book for having "succeeded in what I, or similar to what I tried to do."[148]

Bateson was critical of anything that smacked to him of a therapist manipulating or controlling a client; he found hypnotism uncongenial for that reason and in that respect disagreed with his associate Jay Haley. After reading Haley's retrospective (1976) history of their joint project, Bateson commented in language he would probably not have used twenty years earlier, when their conflict over power had actually occurred:

> Haley slides too lightly over very real epistemological differences between himself and me. As I saw it, he believed in the validity of the metaphor of '*power*' in human relations. I believed then—and today believe even more strongly—that the *myth* of power always corrupts because it proposes always a false (though conventional) epistemology. I believe that all such metaphors derived from pleroma and applied to creatura are antiheuristic. They are groping in a wrong direction, and the direction is not less wrong or less socially pathogenic because the associated mythology is in part self-validating among those who believe it and act upon it.[149]

Haley rejoined:

> This issue of power and control was always a problem within the project. It seemed to me that how much power one person would allow another to have over him was a central issue in human life. It was also a particular issue in our special fields of investigation—hypnosis, therapy, and processes within families, particularly the families of the mad. There was little or no research on power and control at that time, and in fact there seemed to be an avoidance of the subject. The moral issue whether one should or should not *struggle for power* seemed to introject itself into the study of the phenomenon.[150]

He elaborated in an interview:

> Bateson didn't like power. He didn't even like the word. . . . He'd take something I said and turn it into a power issue, when I didn't mean it that way at all. He was oversensitive to the whole issue. [He would have a conflict with] anybody who said "I'm going to change this person." If they said, "I will offer this person some ideas, and if they change, it's up to them," then Gregory would have no trouble with them. . . . Any influence outside the person's range is odious to him. Any direct manipulation is [also] out of the question.[151]

In all this it must be remembered that unlike Haley, Bateson was not a therapist. In fact, he may have disapproved of some of the therapies his own research had spawned. His relationships to schizophrenics and alcoholics at the V.A. Hospital was that of a friend interested in their language, or someone who might play golf with them or take them for a visit outside the institution.

Bateson, McCulloch, and Kubie represented three very different points of view regarding psychiatry: McCulloch believed in the merit of physiological cures, but if they failed, he insisted on not tampering with individuals through other therapies, for the individual and his or her ways, however bizarre, are inviolable. He would offer personal friendship and compassion. Kubie believed in Freudian psychoanalysis, with minor modifications, with its emphasis on mental sickness, personal history as etiology, the high value it placed on consciousness, and its implicit middle-class values. Kubie's and McCulloch's views fell readily within two major traditions and rival establishments of the psychiatric profession. But for Bateson, traditional thinking about psychosis and psychological problems was in need of radical critique and reconstruction. This would lead to an anthropological and epistemological rather than a medical perspective on madness, would favor an interactive social-ecological emphasis over a focus on individual souls or egos, would favor trying out, observing, and reflecting on new approaches, but would eschew direct manipulation of people for their own good.[152] More than either of the other two, Bateson's approach required pioneering in realms where no backing from any major establishment or profession could be counted on. Of course, points of view other than these three are possible, as, for example, the one that Harrower later came to, drawing on elements of Gestalt psychology and humanistic psychology, but these three are the ones most clearly represented at the Macy conferences. Klüver's view, although not articulated at the conferences beyond his penchant for teasing Kubie, is important in that it shows that an etiology of psychosis as rooted in social and political conditions persisted even in those apolitical times. As he wrote in 1949,

> the appearance of psychotherapists in the modern world is itself a symptom of the age. An historical or a sociological analysis may elucidate the various factors operative in producing such a great demand for psychotherapy, but it may also show that these factors are of such a nature as to limit and negate any success psychotherapy can achieve. The real problem may consist, not in adjusting personality disorders, but in adjusting society or an epoch producing such behavior disorders. Even before World War II, some of the Austrian psychotherapists clearly recognized that a true psychotherapy would demand discontinuing psychotherapy for the sake of concentrating all efforts on changing, for example, the norms of penal codification.[153]

Larry Frank, notwithstanding his enthusiasm for individual psychotherapies, whether of the psychoanalytic or humanistic variety, had coined the phrase "society as the patient." Writing in 1947 in a psychiatric journal, he showed that he had retained an awareness of social and economic contexts of psychotherapies:

> There will be more working wives and mothers compelled to work outside of the home if they are to have marriage, a home and children, because apparently our very efficient business and industry either cannot or will not pay wages sufficient to permit a full time home-maker and a full time mother even for children under two. And there will be a housing shortage for several years or longer. It looks highly probable that the instability of family life will continue and probably get worse with more strains and stresses, both from the socioeconomic insecurity and adverse conditions under which people are trying to live . . . and with women demanding recognition of

themselves as personalities and acceptance of their dignity and worth as persons. As we already know the children and youth will show the impact of that.[154]

No record exists, however, of Frank having mentioned socioeconomic causes at any of the Macy conferences.

If one asks which of these differing views is "true," one is begging the question; similarly if one asks whose approach will "cure," one also avoids the deeper issue of what is meant by "cure" and "health" and "disease." The scientific framework of the Macy conferences was too narrow—even counterproductive—for addressing the issues, although it stirred and stimulated them.

Yet the choice each person made, in Jamesian pragmatic terms, mattered. It had consequences for each of them and whomever they influenced. Each outlook entails a necessarily simplified conceptual picture of living and what it is to be human, but one's choice of simplification reflects personal tastes and values (even if they in turn can be traced to social and political conditions), and constitutes in some respects a choice of one's personal future. At the same time each perpetuates, reinforces, or introduces particular values in the society.

Discussion and differences of opinion at the Macy meetings purported to concern purely scientific matters, but that was a gross mislabeling of the true controversy concerning madness. Wherever reliable empirical information was available, Macy participants accepted it without serious controversy. The knowledge of techniques, pharmacological or psychological, that influence human behavior and affect states of mind in more or less predictable directions was readily accepted insofar as scientifically sound studies were available. The concerted effort around 1950 to reduce human affairs to positive science or techniques and technologies led to a mislabeling that made much of what was said and done in connection with psychoanalysis absurd. We encounter a similar bias toward techniques, positivism, and reductionism in various forms at the meetings: in the enthusiasm for interpreting existential issues in terms of mathematical decision theory and game theory; in the mislabeling of the political World Federation for Mental Health as a scientific organization; in the reference to psychology as "behavioral science"; in Northrop's replacement of the actual social and political events by logical systems; in Stroud's technical fix to deal with the dangers of nuclear annihilation. This mislabeling is the earmark of not only the Macy meetings but of the postwar era in the United States.

Under what rubric, then, do the controversies about psychosis fall? The differences among Kubie, McCulloch, Bateson, Harrower, and Klüver concerned norms, evaluations, and ethics. Each would have agreed in the abstract that respect for the phenomenon of madness and respect for the crazy individual are essential. But it is clear that Kubie, McCulloch, and Bateson had entirely different evaluations of what constituted respect. McCulloch's anarchistic attitude was incompatible with Kubie's desire to reform everyone for their own liberation, whereas Bateson's objection to all forms of manipulation except intellectual dialogue entailed yet another norm. Bateson was no physician, and had no vested interest in perceiving psychosis as a medical problem, whereas both McCulloch and Kubie were medical doctors. McCulloch was outraged at the cost and duration of psychoanalytic treatment, but Kubie could take umbrage under the professional standards of psychiatry as conventionally practiced and, especially, as institutionalized by the New York Psychoanalytic Institute. The issues raised by the diverse views of the Macy participants have the makings of high controversy. They (and whatever is their counterpart today) deserve serious attention as questions of ethics, epistemology, and evaluations.

The quotation from Foucault at the beginning of this chapter speaks of using the mantle of science as a source of authority, so as to enhance the power of the psychiatrist and his institution, the asylum, in the domination of the insane. "The art of psychoanalysis," as one of Bateson's close associates, who was trained by him, has with a touch of sarcasm referred to it, also entails systematic domination of the patient by the psychoanalyst.[155] Experts' advice concerning private lives, especially if that advice is institutionalized and based on the masculine authority of science, has been identified by some feminist writers as an important source of oppression.[156] It serves the psychiatrists' professional interest to remain unaware of the elements of domination and politics in their work. When one psychiatrist publicly called attention to it, the profession was up in arms.[157] Although honesty compelled all our protagonists to recognize that scientific knowledge concerning treatment of the insane was meager, it was in Kubie's professional interest (neither

Bateson nor McCulloch was a practicing psychotherapist) to minimize that ignorance. These are political conflicts. Norms, ethics, and evaluations are not politically neutral. The taboo, around 1950, against tracing the origins of psychological suffering to political and economic conditions and seeking to alter them, the "social amnesia" (to borrow Jacoby's phrase) of psychotherapists, was itself part of the political climate.

In closing this discussion I would like to emphasize the one-sidedness resulting from my focus on Macy participants. The focus has been on the self-styled doctors, therapists, and scientific researchers, those in the secure position of dominance vis à vis the patients, the psychotics, the subjects of the various therapeutic activities. The subject's voice is barely heard. We should have heard Walter Pitts's story from his own perspective. That would have illuminated the impact of McCulloch's views on him. We should also have heard the stories of other friends of McCulloch whom he encouraged to rely on themselves and to eschew professional psychotherapists. We can read Tennessee Williams and other of Kubie's literary analysands (or their biographies) for clues to their experience. Bateson made available to the general reader the first-hand account of a nineteenth-century British aristocrat who was treated in "the best lunatic asylums in the country," but the closest we can come to knowing the subjects' experiences to Bateson's approach is to speak to those who were treated by therapists who used some of Bateson's ideas but disagreed with him in other respects—e.g., Jay Haley, Virginia Satir, R. D. Laing, and David Cooper. As shown by his high regard for Alcoholics Anonymous, Bateson recognized that the subjects may know much more than the professionals about how to deal with their condition.

The topic of the problems of deranged minds, artists, and psychiatrists remains wide open. It is a topic for everyone, not primarily for scientists or professionals. It concerns values, ethics, and epistemologies of everyday living. In spite of some relevant empirical information, then as now, especially with the proliferation of diverse therapeutic approaches since mid-century, we have no reliable "scientific" authorities on madness.

Nevertheless, psychiatrists and social scientists in the optimistic postwar mood, forgetting how little they knew but fortified by the notion of circular causality and the atomistic view of society, organized worldwide as a group of experts to make global mental health into a new technocratic ideology.

7

The Macy Foundation and Worldwide Mental Health

Anyone scanning the yearly reports of the major foundations will become aware that their main support goes to projects unlikely to endanger the establishment consensus.

Lewis Coser[1]

The immediate context of the conferences was provided by their patron, the Josiah Macy Jr. Foundation. The large fortune of Josiah Macy Jr., a Quaker, had derived primarily from the Macy family's enterprises in oil (partly in collaboration with the Rockefeller enterprises) and shipping. Josiah Macy Jr.'s daughter, Kate Macy Ladd, had established the foundation in 1930 after a systematic study to identify areas of neglect by extant philanthropies, a study directed by the Vienna-born physician Ludwig Kast, who later became the foundation's first president. The study recommended that health care be the foundation's central concern, but that it emphasize neither direct contributions to relief and service projects nor narrowly conceived medical research. The study found that "biochemical and physiological research were receiving far more attention than the psychobiological and sociological," and what had been especially neglected was "the search for new methods and ideas . . . or operational concepts," and "there appeared to be an urgent need for integration of knowledge and practice."[2] A 1936 review of the Macy Foundation's activities indicates a philosophic orientation by extolling an "organismic approach" and by stating that "'homeostasis,' as proposed by Cannon, is a splendid example of the 'operational concepts' by which biology is reinterpreting phenomena of the living organism."[3] Anticipating by a decade cybernetics' use of the analogy of servomechanisms, the report continues:

> Much as the gyrocompass by internal compensating action maintains the direction of a ship against the buffeting of wind and tide, so do regulating forces within the organism, by delicate responsiveness to outer change, maintain the constancy of the internal environment, which as Claude Bernard has said: "is the condition of free life."[4]

Most of the grants dispensed by the foundation were made to medical schools and earmarked for particular individuals who would direct the research. The foundation gave particularly strong support to investigations in the cross-disciplinary field that came to be known as psychosomatic medicine.

During the war the foundation emphasized research on health problems affecting national defense and cooperated closely with the government. After the war the federal government increasingly took over the support of medical research, and did so on a scale that dwarfed private contributions. The Macy Foundation's effort to find a new niche for itself at that time led to both continued cooperation with government programs and an emphasis on interdisciplinary conferences.[5] The topic of circular causal and feedback mechanisms in biological and social systems, i.e., cybernetics, although cross-disciplinary in the extreme, fit quite naturally into the Macy Foundation Conference Program.

Frank Fremont-Smith had been with the foundation since 1936, when he joined it as director of the medical division. He had begun his career with a Harvard medical degree (1921). His postdoctoral research had been in neurology, much of it consisting of studies in the chemistry of the cerebrospinal fluid. His medical research interest extended to psychiatry. In the twenties and early thirties he was the closest associate of Stanley Cobb, the head of both the Neuropathology Laboratory at Harvard Medical School and the Neurological Service of Boston City Hospital. At that time Fremont-Smith continued with laboratory research and clinical work, but increasingly took on major administrative tasks for Cobb:

> Of all the staff members Cobb's closest relationship was with Frank Fremont-Smith whom he had been encouraging and helping since the early days in 1923 when Fremont-Smith and Forbes completed their paper with Cobb on carbon monoxide asphyxia. Cobb had appointed Fremont-Smith as his administrative assistant in 1925, had arranged for Fremont-Smith's European trip in 1926, and had obtained a laboratory for Fremont-Smith. . . . In the new neurological

unit he provided Fremont-Smith not only with a private office adjacent to his own but also with a well-equipped laboratory area for spinal fluid studies. Moreover, he had assigned to Fremont-Smith Mary Daily, the most creative and enterprising member of the supporting staff. There can be no doubt that Fremont-Smith was bright and was dedicated to his objective of approaching neuroscience from a biochemical base. However, he was not a great clinician, and according to [Edwin F.] Gildea a bit naïve at times in his diagnoses.[6]

Around 1930 Fremont-Smith and Cobb, along with Arturo Rosenblueth and his department head, Walter Cannon, participated in a "neurological supper club" that met monthly. Homeostasis was a prominent topic. By the time he organized the cybernetics conferences, Fremont-Smith had long been acquainted with Rosenblueth and with some of the physiological roots of the ideas underlying cybernetics. In the early thirties Fremont-Smith was also engaged in a study of epilepsy, and a psychoanalytically oriented new immigrant, Erik Homburger, was assigned to his project to investigate the importance of emotional factors in epilepsy. Homburger, who later changed his name to Erikson, became friends with Frank, Mead, and Bateson, and was invited to the third cybernetics conference in 1947.

When he joined the Macy Foundation, Fremont-Smith, under the tutelage of Lawrence K. Frank—another new executive at the foundation—learned how to promote a new subdiscipline. Backed by funds from the foundation, he and Frank worked to create the field of psychosomatic medicine. Drawing on Frank's experience, Fremont-Smith made use of conferences as a means to generate interest. The two arranged for the foundation to help start a professional journal in the field in 1939 (*Psychosomatic Medicine*) and to bring about the incorporation of a professional society (The American Psychosomatic Society) five years later. The journal became the official organ of the society.[7]

Fremont-Smith was influential in deciding to whom the foundation would award research grants. At the cybernetics conferences he functioned mainly as the foundation representative, occasionally enjoining participants to listen to each other, making general remarks about cross-disciplinary communications, and consistently defending psychoanalysis. He was becoming "Mr. Interdisciplinary Conference," making a career of arranging conferences. According to an account published by his friend Harold Abramson, Fremont-Smith organized as many as 350 small interdisciplinary meetings in the thirty-five years following the Second World War.[8] Fremont-Smith had brought Abramson to the Sixth Cybernetics Conference as a guest. Abramson's work highlights the diverse purposes, covert and overt, to which conferences can be put. According to Abramson

> The Macy Foundation, through Dr. Fremont-Smith's influence on its Board of Directors, helped me personally to organize a research project, studying in a scientific way the action of LSD on man and on fish. . . . LSD at that time (1950) was a very threatening compound because it was not known what effect it would have on the human brain in repeated doses. To start a project of this type in an institution, one had to get official sanction. It was necessary to get special sanction from people who were conversant with research, and whose judgment was considered faultless. I therefore asked Dr. Fremont-Smith to approach the hospital where I wished to start these experiments. . . . A multidisciplinary project was accordingly set up. Without Dr. Fremont-Smith's insight into multidisciplinary problems involving mentation and chemistry, I don't think the project could have developed as it did.[9]

Abramsom, who was associated with Columbia University and Mt. Sinai Hospital, fails to mention that he was also working for the U.S. Central Intelligence Agency, which sought "mind-control drugs" to "modify an individual's behavior by covert means," truth-telling drugs for interrogating prisoners, drugs one might put into a city's drinking water to make the population passive, and drugs for other military and malicious uses.[10] The CIA was especially interested in the hallucinogen LSD, and Abramson was deeply involved in promoting LSD research on their behalf and reporting the latest findings back to the intelligence agency. One among quite a few instances that came to light twenty years later concerns a man whose drink had been secretly spiked with LSD by a researcher seeking to observe behavioral effects. The subject, Dr. Frank Olson, became very disoriented, was sent to Abramson, who posed as a psychiatrist, and subsequently leaped to his death out of a hotel room window. An elaborate cover-up was invented to hide the facts from Olson's family. Research continued after Olson's death, finding further unknowing human experimental animals. In this research the Macy Foundation was for a time used as a conduit for CIA money designated for LSD research. Fremont-Smith organized three conference series: Problems of Consciousness, 1950–1954; Neuropharmacology, 1954–1959; Use of LSD in Psychotherapy, 1956, 1959. Aside from their overt scientific purposes, these conferences conveniently (from the CIA's point of view) brought leading contractors for CIA-sponsored drug work together with government people concerned with its application. Abramson edited the ten volumes for two of the conference series.[11] He also introduced Fremont-Smith to the experience of taking the drug.[12] This use of unknowing subjects in LSD research illustrates how scientists' presumed impulse to truth and psychiatrists' presumed impulse to healing can be manipulated, in this case by the CIA, to a point that violates the most minimal standards of human decency.

Fremont-Smith brought another guest observer to the Ninth Cybernetics Meeting, Mottram Torre of the Personnel Division, U.S. Mutual Security Agency. Fremont-Smith explained that Torre would advise him on group dynamics.

Fremont-Smith saw his own role as that of facilitating uninhibited interdisciplinary communication and thereby filling a social need. I interviewed him in 1968 at the Academy of Religion and Mental Health in New York (he was then acting chairman of its board of trustees), and although he was genial, he was so cautious that I got the impression he was anxious to keep something private—ironic for one who believed in "open" communication. Much later I learned of the CIA involvement, which might have been the source of his anxiety (the Macy Foundation's records have not been open for researchers). Something of his views on science, war, psychoanalysis, the power elite, and on his own work are reflected in his comment at the Sixth Cybernetics Meeting:

> The development of effective communication across the scientific disciplines is perhaps the most urgent need of our era. As Dr. Abraham said, until the nuclear physicists, who through their science have developed the ultimate weapons of hostility, can communicate with the psychoanalysts, who through their science have developed the greatest understanding of the nature and control of hostility—until both these groups of scientists can communicate with, i.e., "make sense to," those who are responsible for the administration of human affairs, there will be no hope of applying the principles of science and logic to the problems of social behavior and world peace.[13]

Fremont-Smith fulfilled what he advocated in that he communicated with scientists, psychoanalysts, and government and corporate administrators. But his innuendo concerning nuclear physicists and the "ultimate weapons of hostility," his preoccupation with hostility, is misplaced. Scientists had continued to work on atomic bombs at Los Alamos, even after it was known that no atomic threat from Germany existed, more because of excessive team spirit in an interdisciplinary group, excessive cooperation with the government, and excessive fascination with the purely technical-scientific features of the project itself, but not because of exceptional hostility.[14]

In 1955, when Willard C. Rappleye, president of the Josiah Macy Jr. Foundation, reviewed the first quarter-century of the foundation's activities, he asserted that "social conflicts are actually symptoms of underlying causes" and that psychiatry teaches us the nature of these causes. Consequently, "the insights and methods of psychiatry, psychology, and cultural anthropology" elucidate "the emotional disturbances of the world."[15]

Dr. Rappleye was one among many whose reaction to political instabilities, wars, and economic conflicts among social groups was to psychologize. "Emotional disturbances of the world": this phrase, when used to refer to wars, revolutions, violent racism, oppression, poverty and starvation amidst wealth, nationalistic ambitions, and more, contains a host of preconceptions and ignores socioeconomic and political causative factors. The fieldwork of cultural anthropologists, however, had shown that typical personality structures varied from culture to culture, and, together with a Freudian interpretation of their genesis, suggested that suitable child-rearing practices and "mental hygiene" could generate a society in which social conflicts, war, and oppression would be ameliorated, if not eliminated. Psychology and social studies could provide a scientific basis for a safe and sane world, one with only harmless "emotional disturbances." Here was a basis for optimism about the future, as well as a beacon lighting the way for specific programs of the Macy Foundation. In this forward-looking approach one had relatively little need of history—preoccupation with the past—or for other "nonscientific" disciplines, the humanities.

Many Macy participants held variants of this general outlook at the time of the conferences. Each variant typically entailed an individual's synthesis of several strands of thought, but usually it contained some echo of John Dewey's pragmatism and focus on education, some adaptation of Freud's theories, and some insights gleaned from the work of anthropologists Boas and Malinowski. Usually it was further conditioned by the ideas of Keynesian economics and the American experience of the 1930s, the New Deal of Franklin Roosevelt's administration, which showed that conscious social programs—whether instituted by government or private foundations—could ameliorate the human condition. The greater the understanding of those designing, managing, and implementing them, the more effective such programs would be. Social scientists at the Macy meetings (representing a liberal consensus at that conservative time) regarded it as an obvious truth that research in psychiatry, sociology, and anthropology could provide the scientific knowledge base needed to achieve the desired humane objectives. Cybernetics, information theory, and game theory were to advance "scientific" understanding of people and societies.

The light of psychiatry and anthropology was, however, only rarely focused on the conduct and belief of the social scientists themselves.[16] It is difficult to admit that "scientific" wisdom is an outgrowth of the biases and visions, ideals and desires of a particular group or profession in a particular culture and that the professionals' view is not Olympian. This faith in the social sciences was particularly clearly manifested in the formation of the World Federation for Mental Health in 1948. Frank, Mead, and Fremont-Smith, three of the regular members of the cybernetics group, played leading roles in organizing the federation. The Macy Foundation was among the organizations providing financial support, as were the U.S. and British governments. To some who feared communist world revolution, world mental health seemed a welcome liberal alternative ideology.

At a UNESCO-sponsored conference in Paris in the summer of 1948 the Marxist-Leninist point of view and mental health ideology confronted each other, as the still-fascinating proceedings show.[17] A viewpoint centered on mental health was presented by the American psychiatrist Harry Stack Sullivan. Sullivan had been helped decades earlier by Frank to obtain funding for his projects and had worked with Fremont-Smith, Mead, and Frank to prepare and organize the World Federation for Mental Health (WFMH) that summer. His now-classic essay, read in Paris, was titled "Tensions Interpersonal and International: A Psychiatrist's View." He was an articulate exponent of worldwide mobilization of psychiatry as a means to achieve enduring peace and social progress.[18] His position was congenial to the growing anticommunist centrist liberal consensus among postwar thinkers in the United States.

Diametrically opposed to the mental health focus was that presented by the only Eastern European social scientist at the UNESCO conference, Alexander Szalai of Budapest, who represented a classical Marxist-Leninist viewpoint in which the ownership of the factors of production is seen as central. By its very nature monopolistic capitalism requires wars and oppression; psychologizing while leaving the economic system intact fails to confront the problem. Psychological or social tensions themselves are not the problem—in fact, in some historical circumstances they serve the cause of human progress and contribute materially toward a more just social structure. If an American social scientist had espoused Szalai's point of view, he would probably have found himself in political hot water; correspondingly, if a Soviet social scientist had advocated the mental health approach to international problems, he would presumably have found himself in serious difficulty.

Whereas the classical Marxist-Leninist ignores the depth psychology of Freud, it is incorporated into the dialectical thinking of the "critical theorists" (Frankfurt School) such as Max Horkheimer, who had common ground with both Sullivan and Szalai. Horkheimer and other Jewish Central-European immigrants to the U.S. were much closer to the horrors of Nazism and the Second World War and were willing to countenance fundamental changes in social structure. This group of intellectuals also took Freud more seriously than did Mead, Fremont-Smith, and Frank, who in their humanistic optimism rejected the pessimistic elements of Freud's thought. The Frankfurt Group emphasized that human wishes, needs, and character structures are not root causes, for their form is contingent on socioeconomic class, political conditions, and the form of government. At the UNESCO meeting Horkheimer gave a detailed analysis of the origins of fascism in Germany and Italy, an issue close to everyone's consciousness in 1948, to illustrate his thesis. He commented after Sullivan's paper that

> the intrinsic danger of the "psychiatrist's view" lies, I believe, in the encouragement of a manipulative attitude. . . . We get the impression that the anxieties which Sullivan correctly relates to today's international tensions could be effectively cured by psychotherapeutic measures, whereas the main task, in my opinion, consists in the removal of the objective causes for such anxieties, which are far from purely psychogenic in nature.[19]

Here is no denial of the Freudian unconscious, but only the insistence that if one pursues the origin of psychological "symptoms" and suffering sufficiently far, they are found in socioeconomic or political patterns.[20]

Mead, Frank, Fremont-Smith, and their friends had been talking at various times during the war about postwar concerns. More formal organization had already been initiated by spring 1943, when a group within the National Committee for Mental Hygiene "decided to bring about the organization of a post-war committee, representing several organizations interested in psychiatric and mental hygiene problems."[21] The resulting Joint Committee on Post War Problems and Opportunities elected Frank, who represented the National Committee for Mental Hygiene, its chairman. At an early meeting (9 February 1944) he mentioned an informal group that had already been meeting for three or four months, a group that included himself and Kubie, Fremont-Smith, Mead, Bateson, and Kurt Lewin. Specifically, they had been discussing problems related to proposals for dealing with Germany as a defeated nation.[22] A separate impetus for larger conferences and committees came in autumn 1945 from members of the European Committee for Mental Hygiene, who requested that an international conference on mental hygiene be convened in London in 1947. The International Committee on Mental Hygiene had met twice prior to the war (1930, 1937). In the postwar years participants of the Macy conferences on cybernetics—Fremont-Smith, Mead, Henry Brosin, Molly Harrower, and Clyde Kluckhohn—were on the governing board of the International Committee on Mental Hygiene, and Fremont-Smith was a vice-president and the chairman of the executive committee. The plans for the London Conference were extraordinary, especially the attempt to involve a large number of active participants in the preparation.[23] Three hundred and fifty-one preparatory groups—more than two hundred of them in the United States—composed of over four thousand men and women were set up in thirty-five countries. Each group would send its plans and suggestions to one of five central preparatory committees in London, would be informed of progress by a regularly appearing bulletin, and an international preparatory commission would be selected. The commission, its members housed together, would spend two weeks working out the program, lay explicit plans for the conference, and prepare a joint statement to be offered to the large conference for ratification. The International Preparatory Commission had nineteen members, six each from the United States and Great Britain, one each from Canada and Brazil, and the others from Western European countries. Funds for the program were obtained from the British and American governments, the Macy Foundation, and other private sources. Frank was chairman of the commission; Mead, Fremont-Smith, Harry Stack Sullivan, and Otto Klineberg (psychology professor at Columbia University, associated with UNESCO) were the other U.S. members.[24]

Here Frank, Mead, and their friends could attempt to make some of their grander visions concrete. They debated whether the organization should define itself as scientific or political and chose the former; whether reports of study groups or individual talks should dominate the conference, and decided to give equal time to both. Their concerns are expressed in their report on the work and the statement prepared by the International Preparatory Commission. The report was authored by Frank and Mead.[25] A partial summary, with quotations, follows. A central conception is "that human nature, as we are now discovering, is much more plastic and flexible than has been heretofore recognized . . . the development of personality must be studied specifically in terms of the interpersonal relationships to which the child is exposed." It also is clear "that the social, political, economic, legal and other aspects of society should be reexamined in terms of the dynamic theory of personality." Especially "research must be conducted in such a way that the psychiatrist and social scientist are brought into the closest possible contact with the administrator and political leader. . . . The goal of mental health has been enlarged from the concern for the development of healthy personalities to the larger task of creating a healthy society." (It was Frank, who had many years earlier, first spoken of the society as the patient.) The ideas explicitly entail the notion of circular causality, that society and personality are each other's cause and effect. "The sciences of man offer the hope of a new approach to the problem of war and a world community . . . it is the ultimate goal of mental health to help men to live with their fellows in one world. . . . The concept of mental health is co-extensive with world order and the world community that must be developed so that men can live together in peace with each other."

When the large founding Conference of the World Federation for Mental Health was finally held, the statement was ratified. The motto of the organization, borrowed from the UNESCO constitution, was: "Since wars begin in the minds of men, it is in the minds of men that the defense of peace must be constructed."

In subsequent years Fremont-Smith, Mead, and Frank continued to play important roles in the newly formed federation; Fremont-Smith was president for one year (1954–55) and Mead for another (1956–57), and all three served on the Interprofessional Advisory Committee.

Mead (with Frank one of the working group) was the editor of *Cultural Patterns and Technical Change,* cosponsored by the Federation and UNESCO.[26] The study acknowledged that introducing modern techniques in nonindustrialized countries may generate psychological and social tensions; the book aimed to be a manual describing methods for relieving those tensions. The original WFMH proposal to UNESCO was for a study whose objective was to facilitate and expedite the process of technological change, while protecting and advancing "mental health." The techniques favored in the book for cultivating mental health were the latest technologies; mental health was one more technology—the one that could insure that introduction of other technologies would go smoothly. The implicit premise was that American and British techniques of mental health were appropriate means of resolving the political conflicts generated by aggressive, expanding American capitalism impinging on indigenous societies. The hidden political agenda was the U.S. policy of "containing" communism.

At the Sixth Cybernetics Conference in March 1949 Fremont-Smith told the cybernetics group about the London Conference on World Mental Health, and pointed out that an international and interdisciplinary group in London had agreed on a 20,000-word statement. It is so unlike him to bring news of his other interests to the cybernetics group that he must have felt particularly proud of the achievement.[27] He had earlier sent a copy of the statement to McCulloch, who didn't share Fremont-Smith's enthusiasm:

> I have read, marked and inwardly digested the manifesto of the First International Conference for Mental Welfare. . . . I would take no issue with your specific suggestions except that it seems to me that all of them are leveled at psychological and sociological evils whose biological chemical and physical roots are still almost unknown. Perhaps I am merely too radical at this moment to wish to "Tamper with Government."
>
> But I have a second objection to all theories of perfectability. . . . No matter how we tinker with society or with the physical world, I expect internal conflict engendered by circumstances in which personal and group prosperity are at odds. If mental health depends on eliminating such conflicts, and I believe it largely does, than I do not see how this manifestc even looks in the right direction for a solution to the problem. For that rests on some sort of ability in us to decide questions of value promptly and without remorse. . . . All I can say is that had I been one of the committee who wrote your manifesto, its acceptance would not have been unanimous.[28]

It is interesting that McCulloch identifies mental health with the ability to decide questions of value promptly and without remorse. It is a normative aspect of McCulloch's thinking about the making of choices; McCulloch did not accept the idea of the hierarchy of values needed to apply Savage's decision theory. McCulloch's suggestion that not enough is understood about the "psychological and sociological evils" called "mental illness" to warrant such a grand program, was—alas—valid. In fact, after some years an increasing number of members of the World Federation reached similar conclusions, and the organization eventually disbanded. Incisive public criticism by an American psychiatrist of the premises underlying the WFMH did not appear in print until many years later.[29]

If the phrase "manifesto of the First International" suggests that the organization was congenial to Marxists, that was certainly not the case. It was a liberal humanistic alternative to the Marxist view, as had been highlighted at the earlier Paris UNESCO conference, and only in that indirect sense was it shaped by Marxism.

The response to political and foreign policy issues in terms of mental health or interpersonal relations was compatible with U.S. government foreign policy, even as it represented in modern language and in seemingly scientific dress the traditional

Chrisitian desideratum that love prevail and people's "selves" evolve. The plan to change human character and culture for the better worldwide is an index of prevailing optimism and presumption. It drew heavily on the Christian missionary tradition, most directly in the person of John Rees, the most active British participant-organizer, but was also consistent with Fremont-Smith's inclination to join religion and mental health. As for Frank, although he advocated throwing off tradition, especially inhibiting religious tradition, he remained beholden to traditional ideals, even if they were to be achieved through the "scientific" knowledge of psychiatry and anthropology. For the U.S. government, it was part of an anticommunist program for U.S.–British dominance. For many scholarly and humanitarian participants, the World Mental Health Federation was a well-meaning effort to share with the world our British-American advanced knowledge of mental health. The prevailing anticommunist American mood militated against open dialogue with the Eastern Europeans and Russians, but many social scientists and psychiatrists were eager to use their expertise to "do something" about world problems, and do so with U.S. government backing; for them, the World Federation for Mental Health seemed an entirely natural creation. Yet, as subsequent and even prior history indicates, the effort contained an essential contradiction: The primary, and seething, mental health issue for so much of the world was to find autonomy from Western hegemony. Such men as Mohandas Gandhi and the psychiatrist Frantz Fanon had appraised the psychological needs in non-Western countries far more reliably than had the organizers of the World Federation for Mental Health.[30] The impetus for change had to come from below, and for many Africans and Asians it required opposing such Anglo-American plans for them as the WFMH.

Optimism provides a sense of the future, even as it sustains, guides, and animates activities in the present. But different people feel optimism about different particular directions. McCulloch's optimism seemed to center on creative and powerful minds, affection for particular people and ingenious artifacts, but he seemed pessimistic or even fatalistic about the political direction of the larger society. Stroud's vision of the future meant full utilization of high technology and a scientific attitude toward everything. John von Neumann pinned his hopes for the political future on technical innovation, and, aside from computers, in particular on new weapons technology. Frank's optimism turned on the social benefit to be derived from some of the latest ideas of his contemporaries engaged in "scientific" anthropological and psychological research, and his optimism encompassed the personal and the global. Bateson was optimistic about the adventure of observing human and nonhuman events and learning to understand "the patterns that connect," but—in contrast to the WFMH group—he was skeptical about the potential benefits of social action or any kind of manipulation of people. Norbert Wiener's first commitment was to intellectual honesty, and that seemed to foreclose any long-term optimism, but a philosophic stance encompassing a sense of the tragic was congenial to him. Like Fanon, Wiener understood that intrusion into other cultures, including even anthropological research, is not harmless.[31]

As if to negate feelings of helplessness and anxiety over the new danger of nuclear war, the mood at the cybernetics meetings was one of building hope for the future on science and technology on the one hand, psychologism on the other, and planning for an imagined Pax Americana. This variety of optimism had a powerful part in shaping the future we now live in.

Let us now examine how the broader political and social contexts were manifested in the substance of the cybernetics meetings. A verbal consensus among elite groups in various disciplines evaluated and directed social science research—but these groups were made up of men and women of their own time. Any effort to disentangle the external influences from substantive internal ones is fraught with hazard; nevertheless it serves a heuristic function for historians of social science.

The influence of cold war attitudes and conditions is apparent in the formation of the self-defined "scientific" World Federation for Mental Health, even though many of the underlying notions were not new. (The idea of the malleability of very young human beings goes back to Freud; the culture-and-personality theme had been discussed among American anthropologists well before the Second World War; and organizations devoted to fostering mental health had existed prior to the war.) The necessity to organize support for an alternative ideology to Marxism, the grandness of the vision, the large number of participants, the amazing self-confidence and optimism of the American social scientists and psychiatrists, as well as the availability of governmental financial support for organizing the WFMH, were unmistakable earmarks of the period following the Second World War.

In the case of the cybernetics conferences we can list some ways in which the postwar setting contributed to shaping events.

Interest generated by the Pitts-McCulloch model, its proper reception after the war, indicated a change from its near-neglect in 1943. The change had followed on wartime developments in high technology, especially computers, which exemplified the model in some respects. Postwar enthusiasm for the mechanical and the technical (which had served so well in wartime) was particularly high in the United States. The wartime structure of funding through the military did not change abruptly, but came to incorporate a broader group of scientific and technical projects of theoretical or general interest. The general elation about—indeed overestimation of—the potential of the methods of cybernetics reflects the optimistic belief in technology so characteristic of the period.

The close association of some conferees with government and military projects tended to favor models of mind and society useful in military thinking.[32]

The science and technology of cybernetics were seemingly apolitical, neutral, and objective. For the social scientists, mechanistic models filled the need to be, or at least appear to be, more scientific, a need reflecting the overvaluation of the sciences and the widespread tendency to avoid fundamental political controversy.

Because of the cold war atmosphere no one at the meetings ever advocated or described Marxist social science, even though Marx's analyses of social process, like the theory of the Maxwell governor and Claude Bernard's *milieu interne,* had protocybernetic elements.[33] Political scientists and economists were not invited, although the conferences, as their title shows, purported to deal with the social sciences. One reason for the lopsided emphasis on psychology and psychiatry is doubtless that the Pitts-McCulloch work dealt with individual minds, not society. However, the Rosenblueth-Wiener-Bigelow work and notions from communications theory and game theory were in principle equally applicable to the whole society. A philosophical or political bias is in evidence here, which contributed to putting the emphasis on the individual, the elementary atom, and by comparison neglecting the whole society as a unit. One exception, in which a whole society was considered, was Theodore Schneirla's presentation, at the second meeting, on communication by means of chemical and tactual stimuli in a society of army ants. The machinelike organization of ant colonies could not provide a model for serious consideration of complex human societies.

The response of sociologists and some others at the meetings to political conditions is of particular interest. Only Lazarsfeld and Parsons, both sociologists, considered the larger society, but Lazarsfeld's avoidance of fundamental theories and Parsons's predilection for equilibrium, stability and status-quo descriptions of society both reflected the conservative era.

Societal influences were not limited to the impact of postwar political circumstances. Other aspects of society, such as, for example, the traditional relation of concepts of good science to ideals of masculinity and femininity, can serve to describe the events. The engineers, mathematicians, neurophysiologists were engaged in proper "masculine" science. In McCulloch one notices a strong motivation to turn the "soft" topics of psychology and psychiatry into "hard" masculine formal logic and science. Kubie's effort to give form to the "feminine" practice of psychoanalysis in terms of the respectable idiom of hard science is patently evident in his presentations at the meetings. The theme pervades the cybernetics meetings insofar as they deal with social and psychological subject matter. The effort was always to give mathematical form, to simulate by a machine, or in other ways to resemble engineering when speaking of anything human, even the most personal feeling.

8

Lazarsfeld, Lewin, and Political Conditions

Cybernetic ideas can be applied in the most diverse ideological contexts. Whereas some writers have used the concepts as a language to promote centralization of power, Wiener used them to argue passionately against the concentration of political and administrative power, and to extol the merits of small interactive communities.[1] Social thinkers in Communist countries adopted cybernetics, after having initially rejected it, as entirely consistent with dialectical materialism, and useful in particular for describing social and political patterns that reconcile decentralization of control with overall central purposes.[2] American sociologists have used it to elaborate the functional analysis of the status quo. In applying the language to society, sociologists have not emphasized analogues of the Incompleteness Theorem of Gödel or the paradoxes of Russell, as Wiener and Bateson did. Consequently, descriptive schemes easily became artificially rigid and deterministic.

At the Macy meetings, as the unedited transcript shows, the political conditions were discussed explicitly from time to time. Some participants were government consultants who worked on "classified" topics kept secret from other researchers; their priorities were such that they skipped attendance at the conferences whenever the government called. McCulloch described the situation at the beginning of the ninth meeting in 1952:

> I would like to say that two things have interfered with our gathering this time. One of those is an increasing source of anxiety to me. . . . Thing after thing that one or another person has wanted to discuss at this meeting has been locked up for "secret." I have no idea how far that process will go in time to come. I know that von Neumann had something he wanted to talk to us about and that it is secret. I know that some stuff that Bavelas wanted to talk about to us has become secret. And so it goes.

> The second thing is that I would like to speak with real regret of our losing too many members to interfering engagements elsewhere. We have tried to dodge that as best we could by selection of our dates, by notifying everybody a year ahead of time and reminding them. . . . Evelyn Hutchinson will probably be here for a while but I know he will have to go to Washington. Bavelas is in Washington. Von Neumann is in Washington. I don't know of any way we can avoid it. If any of you have any bright ideas, please tell me.[3]

Aside from advising the government, human scientists responded to the social issues of their time in one way or another: Paul Lazarsfeld and Kurt Lewin addressed and analyzed American social conditions as part of their work, and this chapter will explore their professional responses to particular social-political issues. First we give some attention to Parsons and Kluckhohn—who attended the extra session on cybernetics and society—and their responses, at Harvard University, to Cold War political conditions.

As senior social scientists in administrative positions at Harvard, Parsons and Kluckhohn encountered the practices engendered by the Cold War climate not only as intellectual issues but as practical matters.[4] Parsons dealt with academic-freedom cases because he was president of the Harvard chapter of the American Association of University Professors. Kluckhohn was of particular interest to the FBI because he served as director of the Russian Research Center supported by the Rockefeller and Carnegie foundations. (Mead was director of studies in Soviet culture at the American Museum of Natural History in 1948–50 and so was also of interest to the FBI.) The Russian Research Center at Harvard was set up to aid the American government in foreign policy matters, as well as to provide information for scholars. In spite of Kluckhohn's desire to use social anthropology, psychology, and sociology—relatively new approaches, from the government's perspective—at the Russian Research Center, he accommodated expectations and secured recognition from government officials by putting the major emphasis on political science and economics, the traditional kinds of Soviet studies.[5]

To this day Harvard keeps major portions of its archives about that period hidden from the eyes of scholars, but it is evident from FBI files that in those years some kind of secret arrangement existed between the university and the FBI for sharing information, including data on political beliefs and political activities of faculty—an arrangement that was potentially embarrassing to Harvard.

By 1949 the FBI had made secret contacts at the Russian Research Center and received regular reports from an inside informant. At one point the FBI had checked out Talcott Parson's wife, who was a secretary in Clyde Kluckhohn's office, but after discovering that she held "liberal views with respect to political and social matters," decided that she would not be a suitable informant. According to a 1951 FBI report, based largely on interviews with informants, part of Kluckhohn's job (although it was privately funded) was "to obtain pertinent information requested by government departments and, within limits, shape the research program of the Center to the needs of the United States." Moreover, in response to State Department requests for more information about a particular area, Kluckhohn "would then suggest to a graduate student at the School that he might do a thesis on this particular problem, making no mention to him of the fact that the State Department was also interested." The FBI furthermore claimed to have collected information about Kluckhohn that, they believed, "could, if leaked, have subjected him to humiliation."[6] By 1954 Kluckhohn "had had enough"[7] and resigned as director of the center to again devote himself to his primary interests in anthropology.

Parsons was also deeply interested in the Russian Research Center. He worked with Army Intelligence and the State Department, circumventing government policies to recruit Russian-born Nazi collaborators (one wanted as war criminal by the Soviet Union) from Germany to work at the Russian Research Center, though they had been denied official entry into the United States. Parsons did bring one man in 1948, but despite his efforts, and Kluckhohn's cooperation, Harvard would not give the man an appointment, and he got a position at another American university in 1949. Two others were hired by the Russian Research Center, but did not get into the United States. Their job became to collect information from displaced Russians in Germany.[8] Parsons was entirely willing to pursue Cold War political objectives under the guise of disinterested scholarship.

Talcott Parsons had to deal with an incident in 1955, when the Social Relations Department unanimously voted to offer an instructorship to a young sociologist who had as an undergraduate belonged to the Communist Party. But Harvard University made it a requirement for employment that the candidate be willing to name names of other former Communists known to him, if questioned by "any legally authorized investigating body." So he did not take the job. Two years later, interestingly, the witch hunt atmosphere had abated enough that he was appointed to the position. These incidents involving Parsons and Kluckhohn at Harvard illustrate some of the ways the Cold War climate manifested itself at universities throughout the country.

The special session on social science had been suggested by Lazarsfeld at the first meeting. It contributed to the propagation of the cyberneticians' ideas into the social sciences. His hand in planning the Teleological Mechanisms in Society conference, held on 20 September 1946, shows itself in the focus on quantitative methods in social science and in the selection of sociologists Robert Merton—Lazarsfeld's close friend and Columbia colleague—and Talcott Parsons as guests. Anthropologist Clyde Kluckhohn, a professional friend of Bateson, Mead, and Frank, was invited and became sufficiently interested to attend two regular conferences.[9] The special session on social science introduced leaders in American sociology to the new ideas in a small discussion-group setting two years before Wiener's *Cybernetics* made them generally known.

Parsons, Merton, and Kluckhohn had been among those who had attended the influential seminars on Pareto held at Harvard by sociologist and biochemist L. J. Henderson, Northrop's thesis advisor, in the 1930s.[10] Henderson emphasized the concept of system and equilibrium in chemical systems, the Le Châtelier Principle of thermodynamics, and Cannon's notion of homeostasis in organisms, and related them to Pareto's concept of equilibrium in social systems.[11] Some of the ideas of cybernetics echoed Henderson's 1930s seminars. Henderson had boosted the authority of social thought with the prestige of the natural sciences. And twenty years later conferees at the Macy meetings attempted to link social thought with ideas deriving from logic, computers, and communication engineering. In the 1930s, when Marxist social theory was increasingly prominent, the Pareto-Henderson philosophy had represented an alternative conservative bourgeois approach to understanding society.

In 1946, at the time of the Teleological Mechanisms in Society session, Parsons and Kluckhohn were in the midst of a major administrative reorganization of the social sciences at

Harvard.[12] The Department of Social Relations was established in that year, resolving internal conflicts within several departments and providing a hospitable environment for social anthropologists, social and clinical psychologists, and some sociologists. Parsons (who had psychoanalytic training) became the department chairman. This department was congenial to the type of social science fostered by Mead and Frank, rather than that of physical anthropologists and experimental psychologists.

Parsons, the grand theorist and proponent of functional analysis in sociology, had earlier drawn heavily on the contents of the Pareto-Henderson analysis of society and was to incorporate some of the new ideas of 1946 into his thinking:

> Clarification of the problem of control was immensely promoted by the emergence, at a most strategic time for me, of a new development in general science—namely, cybernetics in its close relation to information theory. It could now be plausibly argued that the basic form of control in action systems was of the cybernetic type and not primarily, as had been generally argued, the analogy of the coercive-compulsive aspects of the processes in which political power is involved.[13]

Parsons eventually became an enthusiast for cybernetics and "often expressed himself on the exciting possibilities he sees in the application of cybernetic theory to the resolution of the gaps between the social and physical sciences."[14]

Parsonian functional analysis, often characterized as bourgeois sociological theory as opposed to Marxian sociology, had called attention to homeostatic mechanisms, such as negative feedback, which maintain basic patterns of a social system. Consideration of radical change in which major social structures are altered led Parsons and some of his followers to think about nonhomeostatic processes—such as biological evolution and especially processes entailing positive feedback in which a new development, perhaps opposed to the old, is rapidly amplified.[15]

Clyde Kluckhohn, whose in-depth studies of the Navajo Indian and other cultures of the American Southwest had made him one of the leading anthropologists, was part of the "personality and culture" movement. During the Second World War he had worked with the Military Intelligence Division of the War Department. In his popular book about anthropology written after the war, *Mirror for Man,* he reveals something of his own outlook, several times quoting Mead as well as Frank with approval. He devotes one chapter, "Race: A Modern Myth," to explaining how human differences in personality, intelligence, and other nonphysical characteristics are largely a function of culture not biology, and he attacks racist theories and all forms of race prejudice and racial discrimination. He quotes Boas, "that a clear understanding of the principles of anthropology illuminates the social processes of our own times and may show us, if we are ready to listen to its teachings, what to do and what to avoid,"[16] and proceeds with a social anthropologist's up-beat and inspirational characterization of the United States and its inhabitants:

> Given our biological and material wealth, given the adaptive genius which is the constructive heritage of our peculiarly American frontier spirit, it will be the fault not of angels but of ourselves if our problems are not in large part resolved. The decisive factor will be the extent to which individual Americans feel a personal responsibility. This, in turn, depends upon an intangible: Their total philosophic attitude.[17]

He urges a philosophy of scientific humanism and holding fast to our ideals. It is personal philosophy that matters, but as to political structure, different forms of government, he is in the American mainstream:

> The paradox of unity in diversity was never so meaningful as today. The Fascists attempted an escape from the "frightening heterogeneity of the twentieth century" by a return to primitivism where there is no harassing conflict, no disturbing choice because there is but a single rule and that unquestioned. The Communists likewise promise escape from freedom through the individual's surrender of his autonomy to the state. The democratic solution is that of orchestrated heterogeneity. One may compare a symphony.[18]

The same rhythm: The fascists are bad one way, the Communists in a different way, but the democracies have a good way, is also found in Mead's popular writing. So is the metaphor of an orchestra. One finds variations on these themes—postwar clichés justifying a shift from a war against Germany and Japan to a warlike stance toward the Soviet Union—in Frank's and Northrop's books as well. Forty years later one still hears that jargon, though with a readier acceptance of fascism than communism. The purely systemic view lacks historical perspective and, surprisingly for anthropologists, neglects the diversity of conditions in different countries. Before a warlike stance toward the USSR prevailed, and before the topic had become a cliché, Karl Polanyi (variously labeled as a historical economist or economic anthropologist) had drawn the comparison along different lines: The market society had failed in the West and, anyway, contradicts fundamental social values. Fascism was one response to that failure, but it sacrificed freedom. The New Deal in the United States was a positive initial response to the failure of the market economy, recognizing social values as prior to economic ones.[19] (The methods and solutions of Russian socialism are inapplicable to Western countries because of entirely different historical situations and need not be considered as an option.) The challenge in the West, in Polanyi's view, is to move, with a commitment to freedom, toward a "democratic, humane, and socialist planned society based on communitarian concerns instead of on acquisitive individualism."[20]

With the internationalist ideals attendant to the founding of the United Nations, Kluckhohn and Mead aspired to a world that respected cultural diversity. They apparently assumed, or at least hoped, that U.S. policies would implement such an aspiration. At the same time they cooperated with the government to gather and analyze information about the Soviet Union. "The prime problem of the century," wrote Kluckhohn in a book published in 1949, "is whether world order is to be achieved through domination of a single nation that imposes its life ways upon all others or through some other means that does not deprive the world of the richness of different cultures. World uniformity in culture would mean aesthetic and moral monotony. The anthropologist's solution is unity in diversity: agreement on a set of principles for world morality but respect and toleration of all activities that do not threaten world peace. The anthropologist regards the attainment of this course as tremendously difficult but not impossible."[21] By 1949 the political action and rhetoric of the Cold War and the U.S. foreign policy of fostering economic imperialism sharply contradicted the concept of an "orchestra" of diverse cultures and the ideal of tolerance. (Writing in late 1989, with Cold War rhetoric on the wane, I wonder whether visions of thriving cultural diversity or ideals of actively promoting the ways of the highly industrialized nations will predominate in the coming decade.)

Merton, a lucid thinker and writer who had been a student of Parsons, was less taken with the ideas of information theory and cybernetics than Parsons.[22] He had characterized both Cannon's physiology (1929) and Bertalanffy's biology (1933), later to become "general systems theory"—protocybernetic notions—as a type of functionalism.[23] He sought to avoid political controversy and yet deal with major topics by developing what he called "middle-range" theories (as opposed to comprehensive, grand theories) for describing limited aspects of social phenomena. Some of the middle-range theories were equally compatible with a Marxian or a Parsonian framework, but Merton's approach was nevertheless controversial and subject to polemics among sociologists.

Merton's studies on the interdependence between science and society, begun in the early 1930's with his now-classic doctoral dissertation, have laid the groundwork for a whole field of inquiry.[24] The dissertation was primarily about the effects of society on the practice of science, rather than the reverse. He examined the economic, military, technological, and religious influences on science in seventeenth-century England. For decades intellectual interest in systematic science-and-society studies was limited to a few scholars, mostly Merton's students. But by about 1960 widespread concern that science itself had generated serious social ills spurred the growth of research into the science-society interplay. Merton, as others, tried to regard sociology as an "immature science."[25] The scientific-progress optimism implicit in that viewpoint fails to acknowledge the importance of controversy, dialogue, and value conflicts in any discipline attempting a genuine interpretation of contemporary society.

Next we turn to Paul Lazarsfeld who, unlike Parsons, Kluckhohn, and Merton, was a regular and original member of the cybernetics group. He had been an intellectual Marxist as a young man, but one would never have guessed it from the work he was doing at the time of the Macy meetings. His personal history, his mediating role in bringing cybernetics from the Macy meetings to sociology, and his response to the silencing of "an entire generation of radical intellectuals" are worth recounting.

Born in Vienna to a family with many ties to the intelligentsia, Lazarsfeld had two strings to his bow: "The two diverse passions which dominated the intellectual orientation of the young Lazarsfeld were the politics of socialism and the logical force of mathematical reasoning as applied to empirical inquiries. . . . The Viennese socialist intelligentsia perceived no opposition between Marxism and empiricism. Lazarsfeld's thinking was in the Austro-Marxist tradition."[26] In 1925 he organized a social-psychological research center within the Psychological Institute of Vienna, staffed mostly by young members active in the socialist movement. He had a doctorate in mathematics, an unusual qualification for someone at the Psychological Institute; he took on outside contracts for market research to keep the social-psychological center afloat. The most enduring study at the center combined use of survey data with immersion into the human situation of Marienthal, a village with a largely unemployed population, and resulted in identifying psychological effects of unemployment, quite distinct from those due to poverty per se.[27]

A Rockefeller fellowship brought Lazarsfeld to the United States in 1933, and when in the following year the Socialist Party was outlawed in Austria and members of his family imprisoned, he managed to stay in America. In 1937 he became director of a large Rockefeller Foundation-sponsored research project on the effect of radio on American society; this project eventually grew into the Bureau of Applied Social Research at Columbia University. Although his own work tended to run along the lines of empirical market research and its analysis, he did find a place in his organization for Theodor Adorno, one of the leading neo-Marxist critical theorists from the Frankfurt School and a refugee from Germany.[28] While the war was still in progress, American social scientists were discussing how to pool their know-how "for the intelligent planning of the new world order which now appears inevitable and for the implementation of any plans which may be made."[29] Lazarsfeld contributed his thoughts about how the mass media could play a constructive role. "It will only be effective if we conceive it frankly in terms of a public relations program for an international authority and apply to it all the knowledge we have acquired of popularization efforts in private and domestic areas."[30] Moreover, he said, it is important that the media be controlled by agencies sympathetic to an international authority, lest they promote chauvinistic nationalism.

During the late 1940s and early 1950s Lazarsfeld retreated from any work reminiscent of his activist socialism, although he described himself to friends as a "Marxist on leave." He devoted his energy to his bureau (which through his efforts became fully incorporated into the university structure at Columbia in 1945) and its work in market and survey research. He seemed to be interested in sociological-statistical method rather than content. Socialism in Austria had lost out on account of popular vote, and in Germany the Nazi party had generated popular enthusiasm. One way to address the failure of socialist political efforts is to understand citizens' attitudes statistically and examine the motivations behind the statistics. This idea may have led Lazarsfeld to the belief that humane political movements might succeed better in the future if they were equipped with a quantitative understanding of popular attitudes. The irony here was that the market research for the advertisers who funded Lazarsfeld's research provided techniques for more effective psychological manipulation and exploitation of people. And the study of voting behavior resulted in more effective methods of manipulating citizens by political candidates and organizations—methods that in the long run have had debatable effects on popular democracy. Lazarsfeld and his bureau concentrated on method and were seen as allies of the conservatives. He was described in an essay honoring him in terms of Isaiah Berlin's metaphors as a "fox" (interested in the world's variety) forced by historical accidents to masquerade as a (single-minded) "hedgehog,"[31] and by another colleague as a man whose "career contained a number of paradoxes."[32]

Lazarsfeld became the leading innovator in modern survey research, and an outstanding methodologist of sociology. He saw the Belgian statistician Adolphe Quetelot (1796–1874), author of a pioneering work in applying statistical reasoning to human and social characteristics, as his primary forerunner and the creator of the tradition to which he belonged.[33] Lazarsfeld was driven to identify human motivations—not for particular individuals but for aggregates of individuals. He "was interested in why people voted as they did, why they bought what they bought, why they used particular mass media. . . . He tried, in the study of attitudes and the development of latent structure analysis, to model mathematically what goes on inside the individual to make him act as he does; he led survey research in the direction of analyzing individual action in a social context."[34]

How did the entrepreneurial sociologist Lazarsfeld come to terms professionally with the political intimidation of professors in the late 1940s and early 1950s? A few years after the Macy conferences, the Ford Foundation's Fund for the Republic gave him a large grant to obtain reliable information on college professors' attitudes toward the political criticism to which they and their colleagues had been subjected since the Second World War.

In 1955 Lazarsfeld and a team of coworkers and assistants conducted a large-scale research effort with extensive questionnaires and interviews. The population they studied were the social scientists teaching at U.S. colleges and universities. Lazarsfeld and Thielens, social scientists themselves, cautiously state what they regarded their task to be: "To analyze, as accurately as possible, the feelings of social scientists during the difficult years and to spell out some of the implications of their attitudes and experiences."[35] They did not undertake to make any recommendations for social action, though they recognized that the sponsors of the study wanted information that "could form the basis for intelligent social action."[36] The statistical compilations and the many individual interviews with anonymous informants give a detailed picture of the social scientists' various fears and apprehensions, the types of caution they observed, their attitudes toward political accusations against colleagues or themselves, their political self-censorship and avoidance of controversial topics, and the discrepancies between what they believed in and what they found the courage to act on. The results of the survey appeared in book form in 1958; with its many graphs, some anecdotal material, and extensive discussion of method, it remains a useful record. By documenting the self-censorship and avoidance of controversial topics, the book identifies a problem for intellectual historians, which led one historian to observe:

> Here again, we confront the problem of evidence. Anecdotes abound, but the full extent to which American scholars censored themselves is hard to gauge. There is no sure way to measure the books that were not written, the courses that were not taught, and the research that was never undertaken. Yet, to look at the academic world's self-censorship is to explore only one aspect of the intellectual fallout of McCarthyism. We must also, and more importantly, examine the scholarship that was done.[37]

Alas, we have only the statistical data, not even Lazarsfeld's own replies to the questions in his questionnaire. After the 1960s had washed away the apprehensions of the previous decade, however, Lazarsfeld showed himself as a fox after all, in a little book he wrote for UNESCO, *Main Trends in Sociology*.[38] Although he put special emphasis on survey research, he also gave summaries of all the major schools throughout the world, including Communist countries, and did not shy away from articulating his own view concerning them, thus becoming not only the defender of "abstracted empiricism" (as his lucid critic C. Wright Mills characterized the neopositivist survey research approach) but an articulate participant in the on-going debate within sociology. He noted, with an underlying managerial concern, that "probably in every country, sociologists disagree among themselves as to the type of sociological work that ought to be done and with respect to the correspondence between actual sociological effort and these ideals."[39]

Critics of Lazarsfeld's style of abstracted empiricism emphasized that the method itself selects problems amenable to it, and that "an empiricism as cautious and rigid as abstracted empiricism eliminates the great social problems and human issues of our time for inquiry."[40] Furthermore, according to C. Wright Mills, the bureaucratic style of that type of social research, as well as the political perspective of its clients, contribute to shaping the qualities of mind of the researchers, and "in so far as such research efforts are effective in their declared practical aims, they serve to increase the efficiency and the reputation . . . of bureaucratic forms of domination in modern industry."[41]

Mills was Lazarsfeld's Columbia University colleague. Undaunted by McCarthyism, he continued to be politically on the left, and was unusual among sociologists of his time in boldly addressing the relation of political power to social structure in the United States. He studied the structure of power and the ways of the relatively small club he identified as an in some important respects corrupt "power-elite"; he examined critically the ascendancy of the military, the mass media, sales promotion, and bureaucratization in the professions; and he pointed out the political indifference of the white-collar middle class. He showed that modern social structures were at odds with citizenship and an effective political democracy.[42] Criticism of the culture on psychological or philosophical grounds was not extraordinary in those years, but systematically examining political repercussions was. He was not invited to the Macy meetings and was never promoted to full professor at Columbia University, but he did remain within the academy.

To place Lazarsfeld and Mills within the spectrum of views about the academic power structure and the Cold War, consider the openly Marxist economist Paul Sweezy, who reviewed Mills' *Power Elite*. While hailing Mills as a "man of courage and imagination, an iconoclast who cares little for the sacred cows of university administrators and foundation trustees, an innovator who wants to get along with the important business of understanding the United States of America in the middle of the twentieth century," Sweezy read into the book implicit socialist conclusions, noting that if Mills had explicitly stated such conclusions the book "would never have been published, reviewed, and read as it has been."[43] Knowing he would not get tenure, Sweezy had resigned from the Harvard University economics department in 1945, dropped out of the academic circuit, obtained a grant, and become an independent researcher and writer. In 1948 he founded the *Monthly Review*. An intellectual Marxist could possibly survive outside the academy if he could find the financial resources. When Sweezy gave a guest lecture at the University of New Hampshire in 1954, however, he was prosecuted by the state's attorney general.[44] In the spectrum of critiques of American society there was a range acceptable to the academies, and for that matter to the power elite, some that were marginally tolerated and others that were beyond the pale. As we shall see, a similar spectrum existed on the more specific topic of racism.

At the Macy conferences Lazarsfeld displayed the narrow focus of a man seeking mathematical techniques for survey research, but he was instrumental in acquainting other sociologists and anthropologists with the ideas of the core group. He proposed a special one-day session for those interested in social science, with other sociologists and anthropologists invited. At the session Lazarsfeld described some of the kinds of statistical analyses he and his coworkers at the bureau had done. In some situations he had found circular patterns: "A certain type of movie attracts a large audience; because of this, more movies of this type are produced; thereupon, people become bored and start to avoid movies of this kind; as a result the producers reduce the supply."[45] The data consists of a time-series of two variables: the attendance at the films and the number of films produced. The two variables are correlated, but time lags and periodicities come into play.

Wiener thought at first that his mathematical theory for prediction on the basis of knowledge of a time series might be applicable to this type of social phenomenon, but the moot point was whether one had collected adequate data over a sufficiently long period of time to make good predictions.[46] Within a year or two Wiener concluded that in the social sciences one is usually dealing with short statistical runs because discontinuous changes in circumstances intervene, and consequently the prediction theory is not promising for application in sociology.[47] For example, in the case of the movie-going data, an unexpected political event in the news could change public taste altogether.

Lazarsfeld was also concerned with influences—mass media as well as personal persuasion—that lead people to make particular choices as voters or consumers, and he presented material on the statistical problems associated with the study of these topics.[48] At the conferences Lazarsfeld was primarily seeking mathematical ways to improve his analysis of the statistical data obtained by the bureau. (Indeed, he later obtained mathematical help from one of the conferees, Savage.) Lazarsfeld did not take much from the cyberneticians into his own work directly, although he did later make use of Bavelas's research based on concepts of message and information.[49]

Academic social scientists' tendency to enlist in the Cold War or withdraw to politically safe topics, or to adopt psychologism, was accompanied by the neglect of major troubles within the United States. The beliefs that all was well in the U.S.A., and that difficulties either emanated from the Soviet Union or could be reduced to private "neuroses," supported a disregard of social and political issues. If at some moments social scientists felt helpless in the face of events that could easily lead to World War III, the optimistic believers in cybernetic technology, or in the World Federation for Mental Health, could provide assurances that they were finding ways to save us all. This sense of the United States as the supremely successful society was contemporaneous with the so-called silent generation of college students who aspired only to private domesticity and a secure job with a large corporation.

The United States was of course no heaven on earth for all of its population, notwithstanding the collective unawareness embodied in the liberal consensus. In 1950 a Marxist economist (who had not been silenced) could call attention to a "report on the distribution of income published by a Congressional Committee in 1949: 25 percent of American families had a total income of $2000 a year. . . . At the same time, government economists noted that over $3000 a year was needed for a satisfactory minimum standard—and nearly half the families in the country weren't getting it;"[50] In 1953 he could assert, "It is not true that we Americans live well. The truth is that while a fortunate few of our countrymen live luxuriously, *most* Americans live miserably. The truth is that 'our high standard of living' is an empty boast—it does not pertain to most of our people."[51] Such statements were vulnerable to being labeled "subversive" or in the favorite epithet of the era, "un-American." J. K. Galbraith, a liberal and compassionate economist, was developing a new economic theory, which, in contrast to the old theories centered on human needs, was based on the observation of the opulence characteristic of people in the United States.[52] Affluence, "where the ordinary individual has access to amenities—foods, entertainment, personal transportation, and plumbing—in which not even the rich rejoiced a century ago," was the great new truth being celebrated. Galbraith did at least acknowledge as a disgraceful fact that the United States still had many poor people; the mainstream of social scientists all but ignored it. They opened their eyes to poverty only in 1962, when Michael Harrington in *The Other America* described the plight of the nation's poor.[53]

The situation of black Americans was another domestic embarrassment. One of the Macy group, Kurt Lewin, worked hard on that issue in his professional capacity. Historian H. Zinn has given a succinct summary of the historical situation:

> The black militant mood, flashing here and there in the thirties, was reduced to a subsurface simmering during World War II, when the nation on the one hand denounced racism, and on the other hand maintained segregation in the armed forces and kept blacks in low-paying jobs. When the war ended, a new element entered the racial balance in the United States—the enormous, unprecedented upsurge of black and yellow people in Africa and Asia. President Harry Truman had to reckon with this, especially as the cold war rivalry with the Soviet Union began, and the dark-skinned revolt of former colo-

nies all over the world threatened to take Marxist form. Action on the race question was needed.[54]

Marxist analysis, whether tied to enthusiasm for the Soviet Union or not, contained a theoretical structure perceived in Africa and elsewhere as a tool for emancipation. Racial equity and communism became further linked because the U.S. Communist Party had been directed by Stalin to support black people against Jim Crow.

In 1946 President Truman appointed a Committee on Civil Rights, which in its report acknowledged a link between discrimination and success in foreign policy:

> We cannot escape the fact that our civil rights record has been an issue in world politics. The world's press and radio are full of it . . . Those with competing philosophies have stressed—and are shamelessly distorting—our shortcomings. . . . They have tried to prove our democracy an empty fraud, and our nation a consistent oppressor of underprivileged people. This may seem ludicrous to Americans, but it is sufficiently important to worry our friends. . . . The final triumph of the democratic ideal is not so inevitable that we can ignore what the world thinks of us or our record.[55]

The Truman administration took small steps toward equality for black Americans, although it promised a great deal more. When goaded by the Progressive Party candidate, Henry Wallace, in 1948, Truman asked Congress to enact much of the legislation recommended by his Committee on Civil Rights, and then took steps toward desegregation in the military and in federal agencies. The Swedish social scientist Gunnar Myrdal, like a latter-day Tocqueville, provided the most trenchant insight into the relations of black and white in the United States, and his book *The American Dilemma* (1944) attracted worldwide attention.

For some American sociologists world criticism provided a political pressure. As one of them wrote in 1957: "As defenders of an alternate mode of life to that proposed by the Communists we are under additional compulsion to make our mode one which can integrate men of every color and culture."[56]

Kurt Lewin, a German-born Jew, had come to the study of minority groups from his personal acquaintance with anti-Semitism in Central Europe. His abstract formulation of the situation of minority groups made it plausible for him to transfer the analysis of the European experience not only to Jews but to the black minority in the United States. In 1935 Frank had arranged generous funds and a position for Lewin in the United States, so that he would not leave to join the faculty of the Hebrew University in Jerusalem; during the war he had worked closely with Mead on a nutrition project. He had first written about "psycho-sociological problems of a minority group" in 1935, and it became an ever-increasing part of his work.

In 1942 Lewin analyzed the importance of time perspective, a concept borrowed from Frank, contrasting the majority of Jews in Germany with Zionist Jews:

> The time perspective of the numerically small Zionist group had been different. . . . For decades they had tried to study their own sociological problems realistically, advocating and promoting a program that looked far ahead. In other words, they had a time perspective which included a psychological past of surviving adverse conditions for thousands of years and a meaningful and inspiring goal for the future. As the result of such a time perspective, this group showed high morale—despite a present which was judged by them to be no less foreboding than by others.[57]

Odd as it seems politically, from a social-psychological point of view blacks' Marxism, together with black history, have played a role similar to the German Jews' Zionism insofar as time perspective and the attendant higher morale are concerned. Lewin recommended various kinds of action to the victims or potential victims of racial and religious prejudice, action to enhance their strength and integrity as well as alter discriminatory practices. Lewin's style was objective. Whether writing from the viewpoint of a member of a minority or from the perspective of a manager working to ameliorate oppressive conditions, he maintained clarity and empathy. His conceptual framework implicitly emphasized personal freedom and dignity and explicitly paid attention to courage; it was not in the Skinnerian behaviorist mode. Lewin described various attitudes and actions favorable to improving the situation for black people; these attitudes and actions would indeed be significant in the later civil rights movement. At the time of the first Macy meeting Lewin was a consultant to numerous institutions seeking to improve "intergroup relations." He emphasized efficient methods for "social engineering," although in his work for the American Jewish Congress he encouraged legal actions and face-to-face confrontation. In his capacity as social engineer he maintained that change in the black minority's situation must come from below and must involve conflict and disruption of the smooth machinery of society—although he believed the disruption could be mitigated if society could first reduce its oppressive practices.

In the summer of 1946 Lewin worked with the Connecticut State Inter-Racial Commission and set up training groups for leaders in intergroup relations. He developed new, highly effective methods of working with small groups. The National Training Laboratory in Group Dynamics, which has since become a permanent institution, grew out of that program. Lewin noted the importance of heightened self-esteem of minority group members *as* group members rather than as individuals. Although he was a psychologist, Lewin knew that work in small groups alone could not bring change unless international political conditions supported change. "Intergroup relations in this country will be formed to a large degree by the events on the international scene and particularly by the fate of the colonial peoples."[58] He understood the pertinence of economic policies as well. "The effect of a policy of permanent exploitation . . . on the international scene, will hamper tremendously progress of inter-group relations within the United States and is likely to endanger every aspect of democracy."[59] It is noteworthy that Lewin did not reduce the problems of racism, part of a system of institutional cruelty, to individual psychology.[60]

Lewin had a strong personal commitment to confront the social issue and an exceptional understanding, which has been corroborated by subsequent history. He died in 1947; by then the U.S. Attorney General had drawn up a long list of suspect organizations, but the full intensity of the harassment associated with the McCarthy era was yet to come. As part of the loyalty-and-security checks, however, investigators inquired "into attitudes of inter-racial sympathy as evidence relevant to a determination of disloyalty."[61] It is noteworthy that many of Lewin's large group of graduate students and young coworkers became increasingly identified with that aspect of Lewin's work concerned with managerial function and the techniques of group dynamics, but most of them did not connect strongly with the social and political movement that Lewin had anticipated.

The movement for desegregation and civil rights in the mid-1950s brought profound changes to American society. It is possible that expectations created by Truman's rhetoric and promises had held the movement back for a time. The refusal of Rosa Parks, a seamstress, to ride in the back of the bus in Montgomery, Alabama, and the leadership of Dr. Martin Luther King, neither a social engineer nor a social psychologist but a Baptist preacher, gave it momentum. The movement came from below. These events, however, were preceded by a sanctioning of desegregation, albeit in a limited context, when the Supreme Court in the case of Brown vs. Board of Education (1954) had decided in favor of desegregation of the schools. The work of social scientists had also played a role: they were accepted as authorities on the central question of the psychological impact of racially segregated schools on black children. Gunnar Myrdal's book was cited in the decision, and Kenneth Clark, a young black social psychologist who had participated in the Myrdal study and had done fieldwork on the subject, was an important expert witness. The testimony of more prominent psychologists known to favor desegregation was sought, but of these only David Krech came to testify.[62] Krech adopted many of Lewin's concepts and formulations; in the preface to a 1948 social psychology textbook of which he was coauthor he specifically acknowledges "the late Prof. Kurt Lewin, who saw so clearly and with such great insight the relation between 'pure' psychology and 'action.'"[63] Krech represents a link—however tenuous—between Kurt Lewin and the civil rights movement.

Although Lewin took cognizance of the time perspective, his analysis, like most cybernetic and system-theoretic analyses of social situations, was essentially ahistorical. It dealt with forces acting in the present, but could not do justice to the roots of those forces. To highlight that distinction and deepen the picture of the social context, consider a remarkable sociologist, who had the Afro-American historical experience in his bones and devoted his best efforts to dealing with it, but who was not part of the circle likely to be invited to the Macy meetings.

W. E. B. DuBois, born a few years after the American Civil War, a larger-than-life black man, was the author of the pioneering 1899 sociological study, *The Philadelphia Negro.* He had been instrumental in the founding of the National Association for the Advancement of Colored People in 1909 and in organizing the Modern Pan-African Movement in 1919. As an NAACP official and as a consultant he attended the founding convention of the United Nations Organization in 1945. However embarrassing it may have been to his native country, he edited "An Appeal to the World" on behalf of the NAACP to protest the persistence of Jim Crow in the United States and in 1947 presented the appeal to the United Nations. He became active in the world-peace movement, which focused on Soviet-American relations and helped form the Peace Information Center "to tell the people of the United States what other nations were doing and thinking about war." The center circulated the Stockholm Peace Appeal to abolish the atom bomb, statements by Quakers, and so on. In 1950 advocacy of peace was regarded as un-American and raised the suspicion of communism; the Stockholm appeal in particular was denounced by the U.S. Congress's Un-American Activities Committee. The 1949 international Cultural and Scientific Conference for World Peace, held in New York with Dmitri Shostakovich as a Soviet delegate, had been initiated by Harvard astronomer Harlow Shapley and sponsored by Albert Einstein, Linus Pauling, Thomas Mann, Henry Wallace, DuBois, and more than four hundred others in the United States.[64] From the Macy group only Wiener was listed as a sponsor. (Rudolf Carnap, who had been invited to the Macy meetings as a guest, but because of health reasons sent his young collaborator, Bar-Hillel, in his stead, was another sponsor.) Many, including Pablo Picasso, were denied visas to attend on the ground that they were presumed to be communists. Many distinguished U.S.-based intellectuals were sponsors of the Conference for World Peace—evidence that a number of American intellectuals refused to be swept into mainstream anticommunism, were willing to be counted, and saw the issue, as Shapley did, as one of acknowledging the coexistence of capitalism and socialism.[65] Former leftist intellectuals, who had become intensely anti-Soviet and antipeace attacked the conference bitterly.[66] The mainstream press reported on it with malicious distortion.[67] To even hear the Soviet viewpoint was thought dangerous. The hysteria in high places is epitomized by the following sad incident, just two weeks after the conference:

> In the middle of the night, after a fire siren had sounded, a man was apprehended running through the streets screaming: "The Red Army has landed!" It was Secretary of Defense James V. Forrestal. He was flown to the Naval Hospital in Bethesda, Maryland. On May 22, he eluded attendants and jumped through a screened window to his death.[68]

DuBois's political activities resulted in criminal indictment by the U.S. government in 1951 and, in response to that, the formation of a distinguished international committee in his behalf. His indictment publicized his concerns and further embarrassed the government, while through a lecture tour within the country he and his friends raised the funds for his successful defense. Nonetheless he continued to be harassed, as he described in his autobiography.

> Since 1951 I had been refused a passport by my government, on the excuse that it was not considered to be "to the best interests of the United States" that I go abroad. It assumed that if I did, I would probably criticize the United States for its attitude toward American Negroes. This was certainly true. Later the State Department changed its reasons, and refused to issue a passport unless I declared in writing that I was not a member of the Communist Party. As a matter of fact I was not a member of that party. Yet I refused to make any statement on the ground that the government had no legal right to question me concerning my political beliefs.[69]

Like DuBois, Macy participant Kurt Lewin had opposed the mistreatment of minority groups in the United States and had translated his thought into social action, but unlike DuBois, Lewin's activities were acceptable to the liberal establishment, and he was a popular member of the tribe of social scientists associated with Frank and Mead. Lewin had experienced the United States as a haven from persecution, but DuBois knew the country first-hand as an oppressor of dark-skinned people. One of Lewin's theoretical notions was that if someone deviates from the mainstream, forces arise to pull him or her back, but once a person has moved beyond a certain threshold distance, the force is in the opposite direction. In these terms Lewin, the social engineer, never went past the threshold; DuBois did. The direct radical political struggle in which DuBois and a few other social scientists engaged to improve social conditions stands in sharp contrast to the liberal social scientists' approach.

9

Gestalten *Go to Bits, 1: From Lewin to Bavelas*

The most complete and concrete descriptions of situations are those which writers such as Dostoevski have given us. These descriptions have attained . . . a picture that shows in a definite way how the different facts in an individual's environment are related to each other and to the individual himself. . . . If psychology is to make predictions about behavior, it must try to accomplish this same task by conceptual means.

Kurt Lewin[1]

Group experimentation is a form of social management.

Kurt Lewin[2]

The study of psychology can be divided into somewhat overlapping subdisciplines in a variety of ways. One could, for example, divide it by areas of specialization: developmental psychology, cognitive psychology, social psychology, animal psychology, physiological psychology, study of personality, clinical psychology, and so on. Alternatively, psychology can be roughly classified according to the school or theoretical orientation of the practitioner.

At the time of the Macy conferences neobehaviorism was the dominant school of academic psychology in the United States. In its original form, represented by John Watson, behaviorism placed a heavy emphasis on conditioning and in its radical operationalism quite rigorously ruled out the study of "mind." In the 1920s and 1930s the tenets of behaviorism were modified by the neobehaviorists (Hull, Tolman, Skinner) and were challenged in the United States by Gestalt psychology, which had been developed in Europe. A number of American psychologists, notably Edward Tolman and Robert Ogden, welcomed whatever insights they might incorporate from Gestalt psychology.[3] Tolman, for example, broke with strict behaviorism by accommodating the useful notion of means and ends in behavior, even though purpose (or intention) is not directly observed. The 1943 Rosenblueth-Wiener-Bigelow paper reflected a neobehaviorist viewpoint with, however, the introduction of "purpose," a characteristic originating with the organism over and above the stimulus/response framework.

The Macy group did not contain many behaviorists. It did include Donald Marquis, who in the 1930s had worked on neurophysiological mechanisms of conditioning, and was coauthor of a leading 1940 psychology text in the behavioristic mode.[4] In the 1930s when McCulloch was working with Dusser DeBarenne at Yale, Marquis was also at Yale, doing research in neurophysiology in John Fulton's laboratory; McCulloch and Marquis came to know each other at that time. Both had attended Northrop's interdisciplinary seminar on philosophy of science. But by the time of the Macy conferences Marquis was an ex-behaviorist, for the war had shifted his activities to study of attitudes among soldiers and military psychology. When in 1945 he became chairman of the University of Michigan psychology department, he decided to build up social psychology rather than physiological psychology. His own interest had shifted to organizational psychology. After 1947 he created the Center for Group Dynamics by hiring the former students and younger associates of Kurt Lewin, the seminal social psychologist of the Gestalt school. The other Macy participant who could be labeled a behaviorist was T. C. Schneirla, comparative psychologist, who primarily observed the behavior of ants and other animals. His notion was that in comparing species, differences are as important as similarities. He warned against the danger of zoomorphism, i.e., interpreting human actions by analogy to animal behavior, but himself liked to contrast humans with other animals. He was skeptical of the unifying concept "mind," but he acknowledged that various kinds of human thinking, symbolizing, and imagining are functions that may obviate recourse to overt behavior.[5] It follows that nonbehavioristic psychologies would be required to describe these processes. Clearly, neither Marquis nor Schneirla was a doctrinaire behaviorist.

The influence of the Gestalt school and its originators—Wertheimer, Köhler, Koffka—was felt at the conferences, although it was stronger at the early meetings. One of the invitees to the first meeting was Molly (Mary Rachel) Harrower, a longtime friend of McCulloch, who had begun her career as the sole American Ph.D. student of Kurt Koffka at Smith College. She retained throughout her life a strong affinity with the Gestalt point of view.[6]

Kurt Lewin, another conference member, had been heavily influenced by the Gestalt movement. His innovations are frequently regarded as an extension of Gestalt theory from cognitive to social psychology. He was excited about the possibilities of the cybernetics meetings, and in response to the invitation to participate—having talked with Wiener, Bateson, and Mead—was "looking forward with much expectation to this adventure."[7] The group of students and coworkers Lewin had gathered about him in the United States are an impressive lot, who have been credited with shaping the field of social psychology in the following decades. It is appropriate to regard Lewin together with his American students as a new tribe constituting a "center for micro-evolution."[8] Heinrich Klüver, another regular at the meetings, had also been a student in Gestalt psychology, but then evolved his own empirical style of psychology in which Gestalt theory was only one of several efforts to understand human perception.

By the early 1930s Gestalt psychology was a major school in Germany, although it had not yet made significant inroads into American academic psychology. In 1933 Lewin, a refugee, arrived in America. His impact was so strong that he has been described as the "most important immigrant to remodel American psychology" in the thirties, forties or fifties.[9] His influence in America is remarkable, for on the whole the Gestalt group "were seen as intruders, alien to the prevailing psychological atmosphere,"[10] an attitude aggravated by the immigrants' having to compete with American-born psychologists for positions in a tight job market. Institutionally, Lewin remained on the margins of the establishment; he was never given tenure at any American university.

A third school of psychology derived from Freud's psychoanalytic theory and its modifications. Its primary application was clinical, but these clinical notions were sometimes applied to social problems and used with sociological and anthropological data to develop a social psychology. Recall Frank's "society as a patient," and Erik Erikson's studies of various societies from a psychoanalytic perspective.

The McCulloch-Pitts 1943 article had dealt primarily with mind and was completely outside of the behaviorist viewpoint. Its subject was perceiving, knowing, and thinking; behavior was only secondary. These same topics had been studied by the European Gestalt psychologists, who by 1943 had emigrated to the United States and were on the faculty of American universities. While Gestalt psychologists and makers of automaton models agreed that behaviorists tended to ignore some of the most interesting questions of psychology and epistemology, they disagreed about how to approach these questions. Warren McCulloch and Walter Pitts were devout adherents to mechanistic, more specifically electronistic, preconceptions in describing organisms. Their most important achievement had been to show that a central nervous system composed of a network of simple neurons, each having the formal property of an electrical relay switch, could have "memory," could "learn," could "recognize forms," and more. Since neurons are discrete elements with only two possible states, the central nervous system was seen as resembling a digital computer. As Macy participant Heinz von Förster belatedly pointed out, the anthropomorphic concepts such as memory and learning reflect semantic confusion when applied to machines.[11] Thus instead of memory, one could more legitimately speak of recording device, and so on, but the anthropomorphic languages was not challenged at the conferences. McCulloch and Pitts, as well as von Neumann and Wiener, were deeply committed to Kenneth Craik's mechanistic ideology:[12] "To go no further than the 'forces' (of the Gestalt psychologists) is like explaining a railway collision by saying that the two trains were drawn together by a force. It is more fruitful to investigate the mechanism."[13]

The sociological subconference that took place before the second cybernetics meeting had recommended to the whole group that they clarify the concepts of "field" and "Gestalt," which had been used rather loosely at the first meeting, thus obliging them to come to terms with the pivotal ideas of the Gestalt school. At the second meeting, McCulloch reports, The word "Gestalt" came up for clarification and it was at once apparent that five members of the group thought the remaining twenty of us were abusing the term. Before we got through they had convinced us of our ignorance. Hence we decided to let that question rest until such time as we could persuade Wolfgang Köhler to enlighten us. The word "field" had a like freight of frenzy. We seemed uncertain as to whether it was introduced in the sense which it is in physics . . . or whether it was merely hortatory, requiring that we observe wholes instead of setting up mechanistic hypotheses as to components. But we managed to come to no conclusion until such time as we heard from Köhler.[14]

It attests to the intensity of the controversy that those present rejected Lewin's, Harrower's and Klüver's interpretations as hearsay, wanting to hear from Köhler, the originator of the concepts. At that time a distinguished University of Chicago biological psychologist, Klüver, had been a student of Köhler in Berlin and had had a hand in bringing Gestalt psychology to America by lecturing on the subject at Stanford University in 1923–24, two years before Köhler crossed the Atlantic to speak at Clark University and Harvard.

Why was this topic, apparently just a matter of definition of concepts, permeated with a "freight of frenzy" among dispassionate scientists? Scientific concepts are necessarily a creation of the human mind. As the variety of schools of psychology suggests, the available data can accommodate a diversity of outlooks and theories. Typically each school selects a particular type of observation and experiment as central. Which theory becomes prominent at a particular time and place reflects cultural, technological, sociological, and political pressures, as well as the personal inclinations and philosophical orientation of the practitioners and professors. The extrascientific factors that led to the prominence of Gestalt psychology in its heyday, the Weimar years between the two world wars in Germany, were vastly different from those that favored computer and engineering models in the United States during the decade after the Second World War. The cybernetics conferences, as most scientific conferences, excluded by tacit agreement discussion of extrascientific elements; differences in viewpoint were settled under the pretense that extrascientific factors played no role.

The period prior to the First World War in Imperial Germany had been "studiedly hostile to the modern movement. . . .

The universities, in which Germans took such ostentatious pride, were nurseries of a wooly-minded militarist idealism and centers of resistance to the new in art or the social sciences; Jews, democrats, socialists, in a word, outsiders, were kept from the sacred precincts of learning."[15] But it was among these outsiders that new ideas were spawned. Among the marginal intellectuals in Berlin during Lewin's student days there was Georg Simmel. He was treated shabbily by the academic establishment, but was a lecturer popular with students. A man with wide interests, Simmel had given a trenchant analysis of conflict in human groups, and in that regard was Lewin's intellectual predecessor.[16] Some of the marginal men came into their own during the period between the two wars. The immense social and political changes within Germany at the time made these innovative outsiders into welcome insiders—for a decade. Atonality in music, the Bauhaus in architecture, and Gestalt psychology were among the modern developments of Weimar Germany.

In Imperial Germany psychology was not yet recognized as a discipline in its own right: it was a subdiscipline under philosophy. University faculty were civil servants, and funding for academic departments depended on usefulness to the state.[17] Philosophy was well entrenched, especially because it was a required subject in teacher education. The task of experimental psychology was to solve some philosophical problems by means of scientific methodologies. Carl Stumpf, the head of the Psychological Institute of the University of Berlin (the best-funded psychological research center in Germany), fully agreed with this formulation of psychology's task. So did his three students Wertheimer, Köhler, and Koffka—the Young Turks who would create the new Gestalt psychology. (Kurt Lewin also took his doctorate under Stumpf.)

Stumpf was among the pioneers in moving from philosophy to experimental psychology. He had been a student of Brentano and was a friend of Husserl, and had formulated his own variant of philosophical phenomenology, somewhat different from Brentano's and Husserl's, but useful as a guide to his later empirical studies.[18] When in the early 1920s Köhler took over as director of the Psychological Institute, it became the world center for Gestalt psychology, devoted to solving problems of epistemology and cognition by experimental means. Prominent intellectual influences on Köhler and the institute's work included the physicist Max Planck at Berlin University, from whom Köhler learned his concept of science and field physics, and the phenomenologist Edmund Husserl.[19] The ideas of the philosopher of history Wilhelm Dilthey and his students were also part of the intellectual milieu of psychological studies in Berlin, although Dilthey had died in 1911. His name crops up in Köhler's writings, and his approach to psychology was holistic and nonfragmenting.[20] Dilthey had emphasized the individual as a natural unit for study and that as human beings we can achieve considerable understanding of fellow humans by means different from a "dissecting" psychology using methods of the natural sciences. He proposed a descriptive psychology and typology. He also emphasized the total context of a person's mental life, within which particular actions, feelings, and ideas are comprehensible. Dilthey had been a major figure at the University of Berlin and had supported the appointment of Stumpf as head of the Psychological Institute; Stumpf in turn recommended Köhler as his replacement. Like the cybernetics group or the Holderness "invisible college," the Berlin Psychological Institute had been, in Mead's idiom, "a cluster of interacting individuals" and "a unit of cultural microevolution."

As early as 1910 Wertheimer, using Köhler and Koffka as subjects, had noted that if an object or spot of light appears first at one point and a little later at another, an observer may report continuous motion of the object or spot of light. This experience of the "whole," viz. the motion, is different from the experience of its two "parts" at fixed locations. The three young psychologists reflected on parts and wholes: for example, the total sensation and perception of hearing a melody as opposed to hearing the notes and intervals out of which it is constructed. The theme of parts and wholes cropped up in many contexts. The experienced whole, such as a melody or the motion seen from two spots, they termed "Gestalten."

Looking at another person or a piece of furniture, we normally experience the whole—very different from a lot of separate bits of color entering our eyes. The sensory physiology of Helmholtz and the psychophysics of Fechner and Mach had emphasized isolated sensations derived from sound of a particular pitch or light of a particular color, but had ignored the total organization of normal perception. Husserl, in contrast, had advocated analysis of one's actual experience as it is, and the Gestalt psychologists followed suit. In returning to direct individual experience, taking it seriously, and emphasizing meaningful wholes—a melody, a face—rather than fragments, the Gestalt psychologists were in tune with attitudes characteristic of 1920s Germany. As one historian of Weimar Germany wrote, it was a "poetic, philosophical, sociological, and political commonplace—that the modern world was fragmenting man, breaking him apart, estranging him from his society and his real inner nature," and the response throughout Germany was "a hunger for wholeness."[21] Beginning from one's own immediate experience (while attempting to set philosophic or scientific presuppositions aside) and emphasizing wholes reasserted the human. Yet the Gestalt psychologists did not capitulate to mysticism, political reaction, and vitalism, all of which were often connected with the hunger for wholeness. They insisted on empirical studies of phenomena and sought fundamental laws describing structural characteristics of experience, even as they supported the popular opposition to atomistic and mechanistic analyses of the world.

The British-American mechanists had come from a different world than the Central European Gestaltists. At American universities psychology had been justified not in terms of philosophy but rather as a technique for predicting and influencing human behavior. The leading light was behaviorist John Watson, and "conditioning" was the fundamental concept. Subjective experience was entirely rejected by the behaviorists, while for the Gestaltists it was the primary datum. Most of the mechanists at the conferences did not regard themselves as professional psychologists. They were not reading Husserl or Dilthey, or John Watson and B. F. Skinner, but the analytic philosophies of Bertrand Russell and G. E. Moore and Wittgenstein's *Tractatus*. The primary elements of the world are "atomic facts" and language provides us with pictures of atomic facts.[22] They were far more interested in Charles Saunders Peirce's writings than in William James's. And their experimental work was neurophysiology, if they were not computer designers. For them, our power of logical thought—and perhaps our ability to construct complex theories and artifacts—rather than the complexity of our experiences, was what made humans interesting.

Just as Weimar culture was characterized by an antimechanical outlook,[23] so American culture after the Second World War had a pro-mechanical bias. Machines were generally liked in American culture, which was not as rigidly controlled as German. High school kids learned to repair and operate an automobile and thereby experienced an enhanced freedom. The myths of the pioneers' self-sufficiency, Yankee ingenuity and its benefit to humankind, as well as the purported fortune to be made by inventing a better mousetrap, all supported the pro-mechanical bias (although the experience of workers replaced by labor-saving machines or obliged to match the timing of an oppressive production line promoted an opposite bias). World War II, seen as a competition in industrial productivity and innovative technology in which the United States had excelled, and "won", amplified the popular American enthusiasm for the machine. In this climate the concept of the human nervous system as a sophisticated, complex electrical machine did not rouse fears of dehumanization. In wartime it had become natural for some psychologists and engineers in England and the United States to think of humans as part of a larger engineering system, for example, in the design of tracking guns to follow aircraft. As Stroud reported to the Macy group, "So we have the human operator surrounded on both sides by very precisely known mechanisms and the question comes up 'what kind of machine have we placed in the middle?'"[24]

Historians of physics have noted another difference in cognitive style between theoretical physicists in prewar Europe and the postwar American physicists who devised modern quantum electrodynamics.[25] The Americans were more pragmatic, content with theoretical techniques that would lead to useful quantitative results, and less concerned with reformulating the subject in a radical, fundamental way. An analogous difference in style seems to characterize the prewar Kurt Lewin and his American-born student Bavelas, in social psychology.

Kurt Lewin grew up in the Jewish community centered about the synagogue in Mogilno, a village in Germany, now part of Poland.[26] He inevitably got an introduction to a notion of "laws" of human conduct and to the ethical idea of a connection between study and practice from Torah and Talmudic discussion. The institutionalized anti-Semitism in Imperial Germany also contributed to his early education. In 1905 Lewin's family moved to Berlin, where he attended high school. During his first year in college, in Freiburg, he studied philosophy with Husserl, but in 1910 returned to study in Berlin, where he attended the philosophy lectures of Ernst Cassirer, whose roots were in the neo-Kantian tradition.[27] Lewin began his career as a psychologist with Stumpf as his thesis advisor. In addition, with fellow students he organized and taught evening courses for working-class adults, and wrote articles on the psychology of work. By 1919 he had formulated the idea that the application of psychology to work had twin objectives: that the work result in high productivity and be psychologically good for the worker.[28] In a socialist journal, Lewin argued that these rational objectives are the same in a capitalist economy as in a socialist one. The applied psychologist's studies thus may be useful in both types of political systems and in the sense are apolitical. Years later in America when he and his students applied social psychology to industrial settings, it surprised some observers, for it appeared to them that he had let himself be exploited by industrial corporations for their objectives, which were contrary to the workers' interests. However, it is not inconsistent with the ideas expressed in his 1919 article.

In 1921 Lewin was given a junior appointment at the University of Berlin; he remained on the faculty of the Psychological Institute until he emigrated to the United States in 1933. Two of the three creators of Gestalt psychology, Wertheimer and Köhler, were at Berlin, and the new Gestalt psychology was a major source of intellectual excitement at the time. Köhler had taken over from Stumpf as director of the Psychological Institute, and Lewin became deeply involved with the Gestalt movement and closely associated with Köhler.

Lewin incorporated some ideas from phenomenological philosophy into his thinking about psychology, as not only Stumpf but all of the Gestalt psychologists had. The challenge to Lewin, however, was to create a properly scientific psychology dealing with human aspirations, motivations, needs, and tensions—and eventually, society. He rejected psychoanalytic theory as methodologically unsound despite its brilliance.[29] He referred to Cassirer's *Substance and Function,* which analyzes stages in the historical development of the various sciences and their methods and concepts, as his philosophical guide to developing a science of psychology.[30] Lewin was sure that rigorous laws of psychology could be constructed, given mathematical forms, and tested experimentally, thus giving psychology the status of a science analogous to physics.[31] Rather than merely debate this possibility with philosophers, he made it his lifelong task to attempt to demonstrate it. It was a courageous premise, especially in view of an awareness of the history of psychology from the seventeenth century on, for

> modern psychology originated contemporaneously with the physical sciences and has been more or less connected with them throughout this development. To be sure, it has not known the continuity distinctive of the physical sciences. Nor has it known that series of progressive successes which constitute the prestige of physics. . . . The course taken by the development of psychology is much more sinuous. Its development was frequently interrupted as a result of the fact that psychological studies often took directions which, sooner or later, revealed themselves to be blind alleys.[32]

While Köhler and the other Gestalt psychologist made similar sorts of assumptions of lawfulness in their work on perception, they had recourse to the plausible assumption that perceptions directly reflect events in the nervous system and mirror the lawfulness of the physical events. For Lewin the argument was primarily historical. He judged that psychology had reached a new stage—exemplified by the work of the Berlin Gestalt psychologists—which would permit a science of psychology in what he called the "Galilean mode," in contrast to the older, unsatisfactory "Aristotelian mode."[33] Just as Galileo had penetrated to the underlying law of falling bodies as the valid ideal case, however obscured it may be empirically by air resistance or other local, incidental, nongravitational effects, so the psychologist can find laws for correspondingly "pure" situations in psychology. In order to apply these "laws" to actual human situations, the psychologist must develop an unambiguous conceptual language, as rigorously mathematical as practicable, to describe concrete situations. This description of human situations is as important a task for psychology as discovery of laws. In physics the laws do not suffice to make predictions in concrete situations. One must know the specific fields or forces acting on an object, the object's location and motion at a particular time, its intrinsic characteristics, and the nature of the physical boundaries in the environment before one can use the principles of physics to predict the object's subsequent motion. The psychologist's full description of a human situation corresponds to the physicist's specification of a concrete physical situation. The laws must have a character such that consequences can be logically derived from them and be tested by setting up relatively pure experimental situations. If Lewin had merely asserted his fundamental premise of a lawful psychology, it would have been of little interest; it became impressive, however, when it was translated into an extensive and open-ended research program.

Unlike Frank and Mead or Fremont-Smith and Kubie, Lewin thought of a person's behavior at a particular time not as "caused" by his or her childhood experience or personality, but as a function of the current "field" of his or her total "life space," a concept he introduced. His outlook was systemic rather than developmental. Since the life space, or field, was to incorporate all the elements that enter into a person's situation (just as the best descriptions of situations by writers such as Dostoevsky do) the space Lewin envisioned had of necessity a complicated structure.

> What is meant by psychological life space and what must one take into consideration in order to represent it? Certainly one will have to represent the physical environment of the individual to a certain extent, for instance the room where he is and the position of the furniture and other objects that are important for him at the moment; in certain cases also the house in which the room is, the city, and even the country. One will have to represent his social environment, his relationships to other persons, their positions and personalities, and his own place in society, for instance his vocation. At the same time, his longings and ambitions will play an important role, his fears, thoughts, ideals, and daydreams, in short everything that from the standpoint of the psychologist exists for this person.[34]

He gives the example of a prisoner confined to a jail cell, noting that he can have social communication by letter or other channels with friends, family, and lawyers, and that his thoughts and daydreams are not confined to the cell.[35] Thus Lewin distinguishes the various permeabilities of a boundary to bodily, social, and mental locomotion respectively. Lewin attempted representation of such life spaces by diagrams. The life space concept sharply separated Lewin's approach from that of the behaviorists, who excluded all subjective elements. Whereas Lewin included the person and the environment as the individual perceives them, behaviorism tended toward a radical environmentalism in which the person is only an object acted upon, "conditioned."

The notion of field or life space was consonant with the approach of the Gestalt school in its emphasis on wholes rather than parts and in its emphasis on the present rather than on individual development. In writing about psychological field theory, Lewin returns again and again to comparisons with physics. His theory could only be likened to a "unified field theory" in physics that attempts to take account of *all* forces—although to this day no satisfactory unified theories have been devised in physics. And unlike twentieth-century field theories of gravitation, in Lewin's psychological field the emphasis is on snapshots in time, and the rigorous space-time dynamic of physics is lacking. To attempt to include such different spaces as the bodily, the social, and the mental in one diagram is awkward and complex at best, but Lewin was undaunted. Lewin emphasized that even in the simple situation of a person attracted toward an objective the attraction will produce simultaneously action toward and thought about the goal; moreover, behavior is influenced by hope, and one's life space must contain this characteristic.

His perspicaciousness about people, on which colleagues often remarked, provided a touchstone for Lewin's theoretical descriptions of concrete situations. Take for instance his 1941 discussion of "hope" and "morale" (a word popular in those days). It begins with a statement informed by empirical studies of the unemployed, which lies somewhere between a novelist's specific description and a theoretical representation:

> Studies of unemployment show how a long-drawn-out idleness affects all parts of a person's life. Thrown out of a job, the individual tries to keep hoping. When he finally gives up, he frequently restricts his action much more than he has to. Even though he has plenty of time, he begins to neglect his home duties. He may cease to leave his immediate neighborhood; even his thinking and his wishes become narrow. This atmosphere spreads to his children, and they, too, become narrow-minded in their ambitions and dreams. In other words, the individual and the family as a whole present a complete picture of low morale. An analysis of this behavior shows the importance of that psychological factor which commonly is called "hope."[36]

He considers other examples, discusses some quantitative empirical studies, and arrives at a conclusion in accord with common wisdom, namely, "How soon an individual will give up in face of an obstacle depends . . . on three factors: the strength of the psychological force toward the goal, . . . the felt probability of reaching the goal . . . and the degree of initiative of the individual."[37] Empirical studies permit elaboration on these factors, e.g., how past successes or failures effect the felt probability of reaching the goal. Thus both future expectations and past experience enter into the contemporary life space. Over the years Lewin and his students engaged in experimental studies on topics such as levels of aspiration, regression following upon frustration, satiation, interruption of tasks, and the possibility of substituting one goal for another. Results from these studies could inform representation of the content and structure of the life space for concrete situations.

The concept of life space seems an attractive way to begin to construct a conceptual scheme, although one might wonder whether a comprehensive science can be built from this cornerstone without acknowledging its incongruities.[38] Lewin did not follow his focus on the individual life space with a correspondingly deep exploration of the complexity of interpersonal relationships, which entail the life spaces of two or more people.

At the second cybernetics meeting the group, although wishing to hear Köhler's notions, contented itself with listening to Lewin's presentation of his own field theory. Both Lewin and the cyberneticians emphasized systemic description and analysis. And like them Lewin had an impulse to mathematical formulation and representation—although the physics of Max Planck and the engineering technology of the Second World War were not similar paradigms. Lewin communicated effectively with the mathematically-minded at the conference, but his friendship with Mead, Frank, and their associates, and the respect they had for him, also contributed to his standing in the group. He played the social and intellectual role of bridge builder, which he found exciting.[39]

Lewin's work in formalizing social psychology had distanced him from his mentor Köhler, and in 1936 (Lewin was then in Iowa, Köhler at Swarthmore) he explained his position in an open letter:

> Knowing something of the general theory of point sets, I felt vaguely that the young mathematical discipline "topology" might be of some help in making psychology a real science. I began studying topology and making use of its concepts. . . . However, this understanding expanded rapidly, forcing me to consider wider and wider fields of psychology and to face more and more involved problems. . . . Collectives have had and will, I think, always have their place in scientific work. The group which was called the Psychological Institute of Berlin has

> been, I think, such a collective of friends, working together for many years, interested in all fields of psychology, and concerned as much with experiments as with theories. Whether it was valuable, history will show; but at least it was happy and lively.[40]

Lewin has said that he has always been "rather unable to think productively as a single person."[41] In America his first temporary appointment had been at Cornell University in the School of Home Economics;[42] but subsequently he was at the Child Welfare Research Station at the University of Iowa.[43] In 1944 he founded the Research Center for Group Dynamics associated with M.I.T. and at the same time, outside of the academic setting, "he was instrumental in establishing a Commission on Community Interrelations, primarily to deal with the problem of the minority group central in Lewin's concern—the Jew."[44] Wherever he worked, Lewin required a great deal of lively interaction with colleagues and students, and—except perhaps for the brief period at Cornell—was able to attract a group of stimulating and responsive coworkers. He was always well liked. When in 1931 he visited Stanford University for a semester, psychologist Terman wrote, "Faculty and students became so fond of him that it was hard to let go of him. I have known few people who were so alive to everything about them, or so genial and friendly."[45]

In Iowa the focus of Lewin's work shifted, in effect adapted in ways congenial to him to the needs prominent in America.

> Many students who joined Lewin . . . came with a practical interest in the social uses of psychological research. They found . . . Lewin's thinking was strongly life-connected. His theories were tools to attack everyday human problems. They led Lewin, in chicken-and-egg fashion, to place increasing emphasis on experimental studies of the how and what-for of individual and social change—studies which later were consummated as "action research" and "group dynamics."[46]

The series of experiments Lewin and his students conducted on "frustration and regression" with children at play and on autocratic and democratic styles of leadership in small groups were designed to test "pure cases" in accordance with Lewin's concepts of the function of experiment in psychology. They dealt directly with significant questions and became classics.[47] In the context of wartime efforts to—by means of small discussion groups—shift people's eating habits, group decision making was discovered to be a valuable technique, and Lewin held on to that concept.

During the war his mother was murdered in a concentration camp in Poland, some time after Lewin had tried desperately to make the necessary arrangements for her emigration to America. He was deeply affected by his inability to get through the bureaucratic obstacles.

By the 1940s the focus of Lewin's work had shifted to the social psychology of small groups of people and what he called "action-research": research that would be tested and applied in life situations and bring beneficial social change. He initiated the study of "group dynamics," using the conceptual tools of his field theory to describe behavior of people in relation to a group. For social action he found it crucial to go outside of the academic setting and worked with the American Jewish Congress to address in concrete situations race prejudice and other minority group issues. And he created at M.I.T. a center for scientific research on group dynamics placed in the general context of social management, which in particular included research in industry.[48]

When in 1946 Lewin became acquainted with the concepts of circular causality, feedback, and theory of games at the Macy conferences, he quickly applied them to issues that concerned him. His experience in wartime interdisciplinary projects and at the Macy conferences stimulated Lewin to pose new challenges. He asked how best to proceed to integrate the social sciences: for example, to combine psychology with social anthropology and economics. And he asked how to link properly the study of individuals and small groups to large-scale social and political events without loss of scientific rigor.

J. F. Brown, an early student of Lewin's who had been important in making his work known in the English-speaking world, pioneered an effort in the 1930s to extend Lewinian field theory to include sociological-economic-political contexts.[49] He discussed field-theoretical characterizations of state types (liberal democracy, fascist, or communist) and the field structure of membership in religious sects and social classes to supplement the individual characteristics of social-psychological field or life space. Brown insisted that the economic and political background be included in characterizing the individual's psychological world. Just as Horkheimer could see the validity in both Freud and Marx, so Brown found Lewin's and

Marx's formulations compatible. Lewin himself knew the necessity of dealing with political conditions head-on. During the Second World War he had been one of the group of social scientists in the U.S. thinking about the postwar "cultural reconstruction" of Germany. In 1943 on the basis of experiments, Lewin concluded that a change from autocratic to democratic patterns of response and action can be learned in small groups, and suggested "training democratic leaders and leaders of leaders to build up a pyramid which could reach large masses relatively quickly."[50] But such training alone to reeducate Germans was useless, Lewin thought. It was also essential that "the Gestapo or other masters of ten years of terror," be gotten rid of "in a very thorough fashion."[51] In this connection he reflected on the failure of reconstruction after the First World War, which in his view had allowed the reemergence of reactionary forces leading to Hitlerism. "The German move toward democracy after the last war did not fail because the so-called German Revolution of 1918 was too chaotic, but because the overthrow of the Kaiser was entirely bloodless and did not reach deep enough . . . to remove certain sections of the population from power."[52] He added that a revolution in Germany would therefore be something positive. Similarly, in his small group work to improve relations between blacks and whites in the United States (discussed in the previous chapter) he maintained an awareness of the importance of the liberation of colonial peoples in Africa.

Lewin understood all these connections on a practical level. But building a theory that accommodates the complexity of an individual life space and also incorporates large-scale political conditions was another question. Here the notions of circular causality, feedback, steering, and other ideas from the Macy group were helpful.[53] The interdisciplinary character of the meetings was in itself promising. As a step in moving from psychology toward sociology, Lewin recommended regarding groups—not just individuals—as units with group goals and with a field, and considering the interactions between groups. Circular causal and feedback mechanisms were concepts that could be applied equally to individuals and groups of people, even to whole societies. Moreover, since the concepts could be stated mathematically, they were good candidates for formulating an integrated social science in the Galilean mode. Von Neumannian game theory, presumably valid in competitive economic situations, might apply also to noneconomic competitive interpersonal or intergroup situations. Lewin felt that in the atomic age, integration of the social sciences was urgent because separately each social science was deficient and unrealistic (such as economics without psychology or the reverse).[54]

Lewin discovered circular causal processes in many circumstances, such as the interplay between a leader (whether authoritarian, democratic, or laissez faire) and a group. Another circular situation occurs when A (whether an individual or group) dominates or victimizes B; B is then likely to attack, and the aggression escalates unless a limit is reached where the victim gives in. In connection with food: "The large section of the channel which leads from the grocery store into the mouths of the family members or into the garbage can is actually a part of another circular process."[55] Planned social action and the ability to steer that action involves a goal, general fact finding and monitoring, a choice of path, modification of plans and actions in response to information—in short it is a self-regulating process with negative feedback. Lewin noted that social management often lacks good methods of fact finding that inform the next action. Good fact finding can turn social management into action research, and only with such fact finding will managers know the effect of what they are doing and learn what is effective.

The thrust of his work during the last years of his life was in at least two directions: In contrast to the mental healthers, Lewin recognized politics and economics as primary in social change and sought to honor that awareness. But his main effort was to reconcile individual psychology with politics and economics, both practically and theoretically. This effort remains unfulfilled. His other effort was to develop methods of understanding group dynamics and the management of social groups.

An appraisal of Lewin's work and impact is still a subject of dialogue.[56] His personally engaging style, together with his deep insight into human situations, doubtlessly contributed to the impact he had in America. As late as 1947 Lewin wrote, "The combination of experimental and mathematical procedures has been the vehicle for the integration of the study of light, of electricity, and of the other branches of physical science. The same combination seems to be destined to make the integration of the social sciences a reality."[57] (The analog chosen is not arbitrary because the integration of light and electricity is a result of the field formulation of electromagnetism.) But Lewin's field theory and the use of topology and vectors in social psychology have not survived. Explanations vary. For those who insist on viewing psychology as a "young science," an analogy with the epicycles of Ptolemy's planetary astronomy—ultimately replaced by the much simpler Keplerian ellipses about a different center—may have some credibility. Lewin's bold, ingenious theories and experiments are fascinating and important in themselves. Even though recent surveys of social psychology reveal the immense influence of Lewin's concepts,[58] the success in making a case for "laws" (in the Galilean sense) in affective and social psychology has been very limited. One is brought full circle to the philosophical debate about humanistic and scientific studies: Hermeneutics? Literature? Historical and biographical studies? Extrapolation from clinical observations? Small group experiments? Galilean psychology after all? All of the above? Is the object of science to appreciate the world better or to control it?

Phenomenology was one of the many streams of thought Lewin encountered as a student. Where might a consistently phenomenological approach in social psychology have led? Lewin's contemporary Alfred Schutz pursued in a philosophical vein how a social psychology would be constructed along phenomenological lines. Schutz conceived of a "life-world" somewhat analogous to Lewin's life space, but he was not enticed by mathematical representations and concerned himself more deeply with intersubjectivity and a description of the quality of interpersonal relationships. Lewin's topological and vector psychology may have hit a dead end, but Schutz's philosophy may point in the direction where one might continue.[59]

Lewin's work on group dynamics and social engineering is another matter. It codified how to use leadership and group pressure to change individuals' attitudes. In this respect it has been tested and often found successful. Lewin advocated democratic leadership in social engineering, but the very concept is problematic when the objective is to engineer social change.[60] Changes propelled by oppressed members of a society require leadership, struggle, and militant opposition rather than social engineering. To appraise Lewin's group-dynamic techniques is a topic for political philosophy or philosophy of technology rather than philosophy of silence. The philosophy of technology was a neglected field in Lewin's day, although Norbert Wiener of the cybernetics group took up its development.[61]

Kurt Lewin died of a heart attack on 11 February 1947. Correspondence in the archives shows Lewin's close friends' shock and grief and their effort to fill the gap his death had left at the meetings. Leon Festinger (a Lewin student and coworker) and Erik Erikson (like Lewin, a friend of Frank, Mead, and Bateson) were invited to attend the third conference.[62] Neither returned for a second time. Erikson was not sufficiently rigorous to suit some group members, and he found the group's approach too mechanical for his taste.[63] Festinger did not click with the group either.[64] Lewin could not be replaced.

Lewin's former student and coworker Alex Bavelas was brought to the fifth meeting on Mead's recommendation. (Bavelas had worked with Lewin and Mead in Iowa on the nutrition project.) At thirty-five he was one of the younger Macy participants, along with Pitts, Teuber, Savage, and Bigelow. He, it was hoped, would link social scientists and mathematicians as Lewin had, for he combined considerable experience working with small groups as a psychologist and a strong interest in mathematical representation. His 1948 doctoral dissertation under Lewin at M.I.T., "Some Mathematical Properties of Psychological Space," was a refinement of Lewin's mathematical description of life space. Bavelas's formal mathematical model for small groups was as cumbersome and complicated as Lewin's earlier mathematical representations of psychological space.[65]

At Lewin's suggestion Bavelas had initiated small-group experiments at the Harwood Manufacturing Company in Virginia. The project aimed to increase worker productivity while maintaining good morale, and to provide new knowledge of how to achieve this.[66] These studies were successful, and small-group research in industrial settings became Bavelas's forte. He moved with Lewin from Iowa to M.I.T. and soon became a member of the M.I.T. faculty. Upon Lewin's death, at Donald Marquis's invitations, most of Lewin's young protégés moved from M.I.T. to the University of Michigan to form the Center for Group Dynamics. Bavelas remained at M.I.T. until 1956, when he moved to Bell Telephone Laboratories. Then in 1960 he joined the business school of Stanford University as a professor of psychology.

Bavelas had come in spring 1948 to the fifth meeting, which was dominated by linguists. He described some of his experiments in detail at the eighth conference in 1951.[67] By that time he had largely scrapped the complex Lewinian language of life space and replaced it with clean, simple, and usable concepts that derived as much from the notions of information theory as from the elementary concepts of topology. The "purity" of his experimental design, however, was in the Lewinian tradition.

One experiment he described at the meetings involved a small group, typically five. The subjects couldn't see or hear each other, but each could communicate with one or several of the others by sending written, addressed messages through slits in his or her cubicle. Each person was given a card containing five symbols (e.g., an asterisk, a triangle, a circle), and was told that each of the others also had a card of five symbols. The group's task was to find the one symbol (there was only one, as they were informed at the beginning) common to the five cards. This required that somehow they pool information. Bavelas compared efficiency, enjoyment of the task, and learning for different "connectivities" of the group: for example, in one arrangement they were connected neighbor-to-neighbor in a circle; in another, four subjects were connected only to the fifth, "central" subject. In these sort of experiments Bavelas made the concepts of connectivity, information transfer (the number of "bits"), and efficiency precise and mathematical. He also turned the participants into faceless experimental objects without individuality.

Did Bavelas's description of his experiments interest both social scientists and mathematician-engineers at the meetings, as his sponsors had hoped? Wiener and von Neumann were absent at that particular meeting, but mathematician Jimmy Savage, logician Walter Pitts, and mathematically-minded engineers Claude Shannon and Julian Bigelow were immediately intrigued with the puzzle and the game, how it worked and how a group would do it best. They asked if von Neumann's game theory was applicable, but after some discussion concluded that the cooperative as opposed to competitive nature of the game makes it an uninteresting candidate for game-theory application. Was Shannon's information theory applicable? Though Bavelas's use of information concepts was conformable to his own, Shannon thought the problem in communication engineering was a different one and that the core theorems of information theory were irrelevant to Bavelas's experiments.

Donald MacKay, a British guest at the meeting, used the discussion to hint at an extension of Shannon's information concepts that he had been developing, and that he was invited to present in detail later at the conference. The probabilities in Bavelas's experiments are of the subjective type, a fact that seemed to trouble Shannon but intrigue MacKay.

Psychoanalysts and anthropologists discussed those results which showed that subjects consistently made nonrational choices that would violate common-sense knowledge of probabilities. For example, given more information, they played the game less well. Shannon attributed these failings to "psychological factors" not present in communication machines. MacKay made the distinction between human thinking and human logic, but Kubie and Mead were interested in unconscious factors determining irrational choices: a particular symbol on a card may have unconscious meaning that can bias choices; what if a standard symbol is also perceived as a phallic symbol? Partial information, as opposed to no information, may stimulate players to make *ad hoc* hypotheses that will also bias choices, according to Kubie. Kubie suggested another source of bias, "one which is so disturbing that I hate even to consider it. Even if it should turn out to be true, I frankly do not want to believe it."[68] This is the possibility of extra-sensory perception. This is the voice of the "scientific" psychoanalyst. Then the problem of motivation to carry out the task quickly or efficiently (as the experimenter asks) rather than some other way came under discussion. Visiting psychiatrist David Rioch and comparative (animal) psychologist Herbert Birch engaged in discussion of operational definitions of motivation. But then, as so often at the conferences, discussion reverted to comparison of machines and humans. Clearly, one could design a computer that will play each of Bavelas's games or solve his puzzles in the most efficient way, Pitts said, while human groups flounder. After some digression on the anxiety machines produce in humans, Gerard reminded the group that the conscious reason for their interest in machines was to gain clues to how our brains or social groups work. Savage made a nice introspective comment. We know from Turing's theorem that one could construct a machine to do whatever one spells out for it to do. Simultaneously we have two contradictory hopes—to construct mechanical analogues of human actions showing explicitly that it can be done, and that in spite of the theorem, "we might discover some kind of behavior which does not deserve to be called mechanical."[69]

I describe the discussion following Bavelas's paper to give the flavor of the kinds of conversation that occurred at Macy meetings between formal presentations. Ironically, the diverse comments of engineers, psychoanalysts, and others enriched Bavelas's talk and connected it to broader issues—just after Bavelas had succeeded in eliminating the rich content of the social-psychological field in his experiment. For many participants these cross-disciplinary conversations were the heart of the conferences.

Two decades after the event Bavelas told me that the cybernetics conferences had influenced him enormously.[70] In nearly all of his research since that time he has used ideas from these meetings. Elsewhere he said, "I attended several of the Macy Conferences on Cybernetics; they had a profound effect on me in spite of what might have appeared to be no effective communication between the engineers present and the social scientists."[71] Concerning my own interest in the transactions of the Macy group, he told me how he might go about analyzing the transcript. In the kind of analysis he could imagine one would, for example, study quantitatively the extent to which people in discussion refer back to the paper presented or to the previous discussion comment. His suggestion brought home to me the difference between my own approach of seeking to understand the conferences within the framework of an historical narrative, and that of the sociological Bavelas.

Lewin's approach was not historical or developmental either, but over the years at the cybernetics meetings the substantial switch was from Lewin's Gestalt psychology to Bavelas's "bits" of information. As part of his Americanization, Lewin had changed his emphasis from the philosophical or fundamental toward the applied. His students often went even further in that direction. Bavelas's work is a case in point. He seemed to have none of Lewin's grand theoretical aspirations, nor the strong impulse to ameliorate social conditions, but rather sought modest, practical, attainable goals. The generational shift from the European, philosophically trained Gestaltist to his capable American student was considerable.

10

Gestalten *Go to Bits, 2: Köhler's Visit*

> As to the now popular comparisons between human thought and the operation of calculating machines, the greatest reserve seems to be indicated. . . From what he says [Wiener in *Cybernetics*] one does not get the impression that the processes occurring in the machines are functionally comparable to those which occur in human thinking . . . among their functions there is none that can be compared with insight into the meaning of a problem.
>
> Wolfgang Köhler[1]

> Köhler was recording direct current in the brains of his subjects while they viewed sharp-contoured figures passing across the visual field. It was a gallant search for a geometric neural analogue of an experienced figure on ground. Looked at historically, looking back to the early 1950s, it has an almost tragically quixotic quality.
>
> Jerome Bruner[2]

On the second morning of the first of the ten Macy conferences discussion centered on the process of perception. Heinrich Klüver, who had an encyclopedic knowledge of psychological and biological studies of perception, said bluntly that only ignorance exists as to what determines the perception of forms (such as a triangle, the letter A, an oak tree, or a melody). He was thoroughly familiar with Gestalt psychology, but clearly did not believe that Gestalt theories and experiments provided an adequate understanding of the perception of shapes and patterns. Consequently he challenged the assembled scientists to develop a theory of how a brain or—considering the framework of the discussion—how an automaton could perceive Gestalten. In his neuropsychological laboratory Klüver often worked with monkeys, and he may have been thinking not only of humans but also of animals, or of the nascent computer technologies, when he asked how, apart from language, one could know what Gestalten are perceived.

Klüver's question revealed a major area, namely, how we perceive, that was little understood by scientists. The topic lies at the interface of the mental and the physical, and a particular psychologist's approach to it depends on his or her philosophical assumptions about what a human being is. The mind-body problem had been a subject of controversy among philosophers since ancient days, as had the nature of perception. It was a controversy at the Macy meetings as well, especially between Wolfgang Köhler, the most prominent representative of Gestalt psychology, and the cyberneticians, notably Warren McCulloch. Their differences sparked a period of active research in the fields of perception and, more generally, cognition (the problem of how knowledge is acquired) that continues today.[3]

Let us consider the background some of the regular conferees brought to the problem of perception. In appraising work in the field of psychology, its scientific status is invariably a point at issue. Because of his familiarity with its history and his scientific rigor Klüver acted as a kind of scientific conscience to psychology. He did not take the development of physics as his model nor did he view the testing of general theories as the primary function of experiment in psychology. He knew, as he put it, "that psychology has travelled many roads which led nowhere and that it is unique among the sciences in its treasures of *negative* information."[4] His work had shifted from the study of imagery to experimental biological psychology, which, reassuringly, had at least one foot in physiology or neurology. He had concluded that the only route to genuine progress in psychology was narrow specialization, that is, "the penetrating experimental and theoretical analyses of *specific* problems," for "the theories and hypotheses of psychology cover in general too great a range of phenomena and as a consequence become vague and superficial or else ignore the implications of specific phenomena and thus fail to reach true 'generality.'"[5] He therefore was not an advocate of a general approach such as Gestalt psychology or cybernetics. What was he doing in the wide-ranging Macy group? He explained it thus: "Although I have been called 'a lone wolf' I found that these conferences provided for me the *plus* that no amount of reading scientific papers or listening to formal presentations can ever furnish. . . . Those meetings, I am sure, established many scientific and personal ties often not broken for years—or only through death."[6] He was so devoted to his work and his research animals, monkeys, that when at the age of seventy-six he was invited to a symposium at M.I.T., where he was to be honored, he declined at the last minute because his assistant was sick and he had to tend to the animals.

German-born, Klüver had during the First World War been in the German Army, where "he miraculously survived years of fighting, being wounded only once. The rest of his company of 120 men were not all so fortunate. In a year and a half those killed were replaced by approximately 1,500 others."[7] Writing about a friend who'd also been in the war, Klüver remarked, "perhaps even now many of us will understand why some of the survivors returning from battlefields from which millions did not return could feel so deeply that the years that lay ahead were a present, an undeserved present, a precious gift."[8]

In the years 1920–1923 Klüver studied psychology, attending seminars and lectures by Köhler, Wertheimer, and Lewin in Berlin and Stern in Hamburg. In 1923 he left for San Francisco, the sole passenger on a freighter traveling via the Panama Canal, and entered Stanford University. There he lectured on Gestalt psychology and obtained his doctorate within a year. His first job was at the University of Minnesota, where he came to know and admire Karl Lashley, the neuropsychologist. He followed Lashley to Chicago in 1928, was himself sponsored by the Behavior Research Fund, and after five years obtained a position at the University of Chicago. In Chicago he met McCulloch, von Bonin, Gerard, and Henry Brosin, who would all be among the Macy stalwarts.

In his early work Klüver had studied a type of imagery (known as eidetic) that is so vivid that Klüver referred to it as a pseudohallucination.[9] Some "eidetic" individuals will look at a picture and months later the mental image of that picture will appear to them, of their choosing or spontaneously, with all the details in place. The accuracy and stability of detail in such imagery, as in a photograph, are extraordinary. Similarly stable auditory and tactile eidetic phenomenon are also known. Klüver investigated a variation of eidetic images, where only fragments, often relatively meaningless details, of a picture appear and the image itself fluctuates. The fragmentation of the image, Klüver concluded, is not explicable in terms of Gestalt qualities.[10]

"On an October day in 1925, I introduced myself to the world of hallucinogens by consuming some 'mescal buttons' in one of the laboratory buildings of the University of Minnesota."[11] So began Klüver's systematic study of the effects of peyote, described in his 1928 article, "Mescal: The 'Divine Plant' and its Psychological Effects." He reported something of his observation of himself on the drug:

> (Eyes closed) Clouds from left to right through optical field. Tail of a pheasant (in center of field) turns into bright yellow star; star into sparks. Moving scintillating screw; "hundreds" of screws. A sequence of rapidly changing objects in agreeable colors. A rotating wheel (diameter about 1 cm) in the center of a silvery ground. Suddenly in the wheel a picture of God as represented in old Christian paintings.—Intention to see a homogeneous dark field of vision; red and green shoes appear. Most phenomena much nearer than reading distance.—The upper part of the body of a man, with a pale face but red cheeks, rising slowly from below. The face is unknown to me.—While I am thinking of a friend (visual memory image) the head of an Indian appears.—Beads in different colors. Colors always changing: red to violet, green to bright grey, etc. Colors so bright that I doubt that the eyes are closed.—Yellow mass like saltwater taffy pierced by two teeth (about 6 cm in length).—Silvery water pouring downward, suddenly flowing upward.—Landscape as on Japanese pictures: a picture rather than a real landscape.—Sparks having the appearance of exploding shells turn into strange flowers which remind me of poppies in California.—(Eyes open): streaks of green and violet on the wall. The drawing of a head changing into a mushroom . . . etc., etc., etc.[12]

In the midst of such chaos Klüver sought to find order. He acknowledged that the experiences themselves were not describable, and that verbal reports were grossly incomplete. Klüver surmised that in addition to visions, "euphoria is one of the typical mescal symptoms. In spite of marked nausea many subjects 'have a good time'; being in a state of mental exhilaration they become talkative and jocular, they commit social errors and enjoy committing them." Furthermore, "it is true that experiences in the mescal state are not easily forgotten."[13] When analyzing reports of visions under peyote he identified "form constants" within the flow of experiences. He found persistent geometric patterns—honeycomb, chessboard, cobweb, and spirals—to be constants in otherwise rapidly changing visions. These seemed to him useful clues for scientific study, indicating an underlying structure of subjective experience.

In the 1960s and 70s Klüver again commented on peyote and the pure strain of LSD-25 then being produced in the laboratory.[14] By that time LSD and mescal had become well known to the general population and were taken for the sake of consciousness expansion or, more simply, to have a good time. Concerning what seemed to be an ever-increasing demand for drug experience, and a similar demand for psychotherapy, Klüver wrote, "a deeper understanding of this situation would no doubt require a series of detailed historical and sociological studies."[15] Klüver had thus raised an important question for social historians. LSD had already entered the story of the Macy conferences through Abramson and Fremont-Smith, who around 1950 helped the CIA promote interest in and research on LSD, and had incidentally introduced some of the conferees to the LSD experience. Klüver was aware of shifting attitudes toward hallucinogenic drugs since his early researches. It is a brief period in the overall social history: From the time when peyote was a drug used only by American Indians in a limited way, usually in the context of religious ritual, to the period in the 1920s when a few Western researchers, psychologists such as Klüver, were studying the sensory experiences and other effects of the drug, to the late 1940s when chemical knowledge of hallucinogens had advanced considerably and the CIA was promoting these substance for its purposes, to the 1960s when a popular demand for them arose, but eventually subsided and the drugs were then outlawed. A broad perspective requires comparison to the history of other drugs (coffee, alcohol, cocaine, opium), consideration of social attitudes toward heightening enjoyment and toward other than ordinary states of consciousness, economic interests, legislation concerning drug use, as well as knowledge of biological and psychological effects.[16]

Turning from the analysis of reports of visions and imagery, Klüver began working with monkeys in a quasi-behaviorist mode, although he did make surmises concerning their perceptions.[17] He found that a monkey's response to a whole range of stimuli are identical, so that these stimuli are in some way "equivalent." The topic he explored experimentally was the exact nature and extent of a set of equivalent stimuli. He then asked what characteristics these equivalent stimuli had in common.[18] A corresponding question for humans might be, "why the 'same' response is made to the face of a woman, a melody, a rhythm, a 'nonsensical' configuration of lines, a word and a landscape."[19] As in his seeking constancies in peyote visions, so in his study of equivalence of stimuli Klüver was concerned with what mathematical physicists call "invariants" in transformation from one situation to another. On an anecdotal level, Klüver talked at the seventh Macy meeting (1950) of his experience with a baby monkey deprived of attention by its mother, but raised at home by Klüver. It was bedded on a white cotton pad, and later in life the monkey seemed to need at least a small piece of white cotton—even one square inch of material would do—to feel at peace. It was the variety of sizes, shapes, textures, degrees of whiteness of the materials that could be substituted for the original security blanket that interested Klüver. In summarizing the results of his experiments on equivalent stimuli with monkeys, he concluded that their interpretation requires recourse to some "field" properties along the lines postulated by Köhler, Lewin, and in particular Koffka and Harrower. His experiments confirmed, or rather extended to monkeys, Koffka's and Harrower's conclusion that, for instance, color, place, and form are "three interdependent aspects" in perception.[20]

In 1933 Klüver began studying behavioral changes in monkeys resulting from a variety of brain operations. When in 1936 he was given a particularly vicious rhesus monkey, a female named Aurora, he was doing research to determine the location of the action of mescal within the brain. With his colleague Paul Bucy, Klüver intended to study the effects of mescal after the left temporal lobe of the brain had been removed, but discovered that the lobectomy radically altered Aurora's personality and made her a tame creature. This led to further study of the relation of brain to personality and what came to be known as the Klüver-Bucy syndrome. When the hippocampus and the amygdala are removed from the cerebral hemispheres of a macaque monkey, it undergoes a total change of personality. Normally either viciously aggressive toward humans or inclined to hide from people, these altered monkeys become docile, put everything possible into their mouths, display indiscriminate and increased sexual activity, and show none of the normal simian emotions. As regards perception, these altered monkeys suffer from a "psychic blindness"; while they can avoid obstacles and pick up objects, they have lost the ability to "see" the meaning of an object, say, to recognize what is edible or dangerous. Only by putting the object in its mouth can the monkey know whether it is edible, dangerous, or whatever.[21]

Klüver's research requires a few metacomments. He chose to study the abnormal circumstance and the exotic in perception (eidetic imagery, mescal visions, lobotomized monkeys), rather than normal, everyday perception. Yet he used his researches to raise deep issues. That was his style.

It is appropriate for a historian to call attention to the relationship between a scientific researcher and the objects, animals, or people that are being investigated. We have noted the character of that relationship for some psychoanalysts, for some anthropologists, as well as for other researchers. For the kind of work Klüver was engaged in, it meant that even if the researcher had developed affection for particular animals (whose brains in many respects resemble a human central nervous system), he will nevertheless subject them to personality-altering brain operations. That aside, Klüver's devotion to pure science for knowledge's sake, not for application's, neglects the fact that others will use that knowledge. He was skeptical of psychiatry and psychotherapies and did not urge psychosurgery as medical treatment. His view was that "the real problem may consist, not in adjusting personality disorders, but in adjusting society or an epoch producing such behavior disorders."[22] But some psychiatrists took a cue from the Klüver-Bucy syndrome and engaged in horrendous psychosurgery to "improve" the behavior of aggressive psychiatric patients, turning them into near-vegetables.[23] That is the problem of scientific knowledge.

At the meetings Klüver posed the classic problem of how we perceive forms. The Gestalt psychologists had through their theories and observations amplified the importance of that question to understanding perception. Before Gestalt psychology the elementary unit of perception had been conceived as deriving from a necessarily elementary physical process. One neural element, for example, a small, bounded region on the retina, responds to a physical stimulus in a well-defined way, and the resulting sensation is fully determined by that stimulus (the so-called constancy hypothesis). The stimuli were presumed local and totally unaffected by excitation of other, neighboring neural elements. Such has been the nineteenth-century concept of perception based on Helmholtz, Fechner, E. H.

Weber, and on G. E. Müller in the twentieth. But twentieth-century Gestalt psychologists started with the organized units of perception, such as shapes in a field of vision, and these were the elementary entities they needed to understand. How we understand forms became the basic question in the psychology of perception.

Did the cyberneticians bring new tools that could be useful in addressing that question? The concept of information, which Wiener had introduced early at the conferences, was promising for its likely heuristic value—although whether its quantitative formulation was suited to discussing perception seemed doubtful. Wiener spoke in 1947 of the transmission of visual information and its transformation by the nervous system. The Rosenblueth-Wiener-Bigelow notion of an organism engaged in purposeful activity, making use of feedback loops to guide its actions, was also a potentially fruitful concept: Perception would be put into the context of purposeful activity. It would be a segment of the overall feedback loop. Wiener discussed the "eye-muscle feedbacks" in man:

> The human eye has economically confined its best form and color vision to a relatively small fovea, while its perception of motion is better on the periphery. When the peripheral vision has picked up some object conspicuous by brilliancy or light-contrast or color or above all motion, there is a reflex feedback to bring it into the fovea. This feedback is accompanied by a complicated system of interlinked subordinate feedbacks, which tend to converge the two eyes so that the object attracting attention is in the same part of the visual field of each, and to focus the lens so that its outlines are as sharp as possible. . . . Later processes occur in the eye and in the visual cortex.[24]

For Wiener an understanding of information processing by our sense organs was a preamble to tackling the problem of designing electronic or mechanical prosthetic devices, "the problem of replacing the information which is normally conveyed by a lost sense by one which is still available," for example, recreating the equivalent of visual organization in the realm of hearing, so that "objects which ordinarily look alike will now sound alike."[25] In 1947, conscious of the injuries produced during the recent war, Wiener hoped that the problems of sensory prosthesis were not necessarily insoluble.

Walter Pitts and Warren McCulloch addressed Klüver's challenge directly. With financial backing from the Macy and Rockefeller foundations, they were seeking ways to move on from their grand 1943 proof, that in principle a finite network of even highly simplified model-neurons is capable of realizing anything that can be stated unambiguously and completely in words. The next step beyond the general existence proof would be to construct detailed hypothetical neural mechanisms for particular activities of the brain, and if possible give the hypothesis such a precise form that it could be tested experimentally. The problem Klüver had posed would be a good case in point. As a first step McCulloch and Pitts formulated a plausible and specific hypothesis for the neural mechanism by which the brain recognizes a particular geometrical figure, such as a square or triangle, regardless of its size or orientation; and how it recognizes a particular chord or timbre regardless of pitch. Presumably, the same principle could be extended to perception of an oak tree or a familiar face. McCulloch gave a progress report on this work at the third conference, and a few months later he and Pitts had it worked out sufficiently to publish.[26] The requisite automaton would produce the same output, say, a square, for every kind of input that fit the concept of square. Using the mathematical operation of averaging an input over a group of transformations, a brain-automaton could in principle recognize a square or a chord. McCulloch and Pitts went further and identified anatomically the group of neurons in the brain where these operations might be carried out. One of their results is that the distribution of neuronal excitations produced in perceiving a figure, and which represents the figure, need not resemble it in any simple way. The link between the two could be formally expressed as a code. In the model the process of averaging over a group of transformations entailed a scanning operation with a characteristic cycle, which ought to be detectable experimentally, thus providing a means for testing the correctness of the hypothetical mechanism. Walter Pitts and Warren McCulloch wrote:

> We have focused our attention on particular hypothetical mechanisms in order to reach explicit notions about them which guide both histological studies and experiment. If mistaken, they still present the possible kinds of hypothetical mechanisms and the general character of circuits which recognize universals, and give practical methods for their design.[27]

The last phrase in this 1947 article contains a sidelong glance at the possibility of designing devices to recognize figures and patterns of various kinds. McCulloch did not fool himself into believing that artificial intelligence could answer questions on how the brain works. At best it might suggest possibilities. His attitude is clear in remarks he made after an impressive talk by the designer of the logic of computers, John von Neumann: "I confess that there is nothing I envy Dr. von Neumann more than the fact that the machines with which he has to cope are those for which he has, from the beginning, a blueprint of what the machine is supposed to do and how it is supposed to do it."[28] Referring to his wartime experience (experience he had in common with Wiener, von Neumann, and Lorente de Nó) attempting to figure out the purpose and workings of German military devices that had fallen into Allied hands, McCulloch continued: "Unfortunately for us in the biological sciences—or at least, in psychiatry—we are presented with an alien, or enemy's machine. We do not know exactly what the machine is supposed to do and certainly we have no blueprint of it."[29] Lettvin has provided further perspective on his friends' hypothesis concerning the perception of forms:

> Once it was realized in the nineteenth century that nerve fibers conduct electrical pulses and that the pulse trains on these fibers carry meaningful messages, the problem was to account for how such information was processed by the brain as nerve net. Both excitation and inhibition had been shown as nervous actions before 1900, but it was not until David Lloyd's work in 1939–41 that the direct monosynaptic inhibitory and excitatory actions of nervous pulses were demonstrated. This finding, more than anything else, led Warren and Walter to conceive of single neurons as doing logical operations (à la Leibnitz and Boole) and acting as gates. Their concept was the first and most inspired attempt at a theory of nervous action that transcended Sherrington's earlier . . . model. . . . Once this idea of neurons as logical gates was clear, Warren and Walter had to lay out the designing of them and then the ways of connecting them for specific operations; and this they did in engineering style with great verve. . . . That ambition—to show how mental process is sustainable by nervous mechanism—comes to its height in the paper on how universals might be perceived. . . . Walter and Warren, with gleeful hubris chose as example not some simple reflex . . . but rather what can be called a "higher function" processed by an unutterably complicated system. It doesn't matter whether the mechanism they described actually occurs. That is beside the point. What matters is that such an engine can be

designed using the methods they had laid down. They would rather have been clearly wrong than maunderingly vague, as was the accepted style.[30]

Molly Harrower, another regular, had a strong background and interest in perception. Born in Johannesburg, South Africa, Harrower got her education in England and became assistant to Charles K. Ogden (known for his work in developing Basic English and the book, with Richards, *The Meaning of Meaning*[31]) at Cambridge University. She then went to America to work with Kurt Koffka at Smith College. As a graduate student and research assistant she did a series of experiments with Koffka on the interplay of color, brightness, background, and form in perception. The theme running through these experiments is that all these features are nonseparable aspects of a single event of perceptual field organization.[32] She was the only student at Smith to complete her doctorate under Koffka.

Subsequently, under Rockefeller Foundation auspices, Harrower became associated for several years with neurosurgeon Wilder Penfield's experiments in Montreal. Penfield electrically stimulated parts of the exposed cortex of a patient in brain surgery, while Harrower, the psychologist, observed the actions and (voluntary or involuntary) words of the patient. (As she recalled, however, her scientific interest would give way to concern for the person whenever the patient showed signs of distress.[33]) Often the electrical stimulation evoked specific sounds, memories, visions, or dreams. These studies strongly suggested precise localization of memories, "flashbacks," within the temporal lobes. After completing her tenure in Montreal, Harrower began, as a Josiah Macy Fellow, to develop the usefulness of the inkblots known as the Rohrschach test. In her style of using projective tests, as well as in her later work as a clinical psychologist, she retained a Gestalt orientation, emphasizing meaningful wholes and patterns.[34] During the war she was consultant to the U.S. Army and the State Department in connection with using projective techniques on a large scale to help screen and select personnel for the military and other wartime government activities.

Harrower met Fremont-Smith and Larry Frank in the 1930s and Kubie in 1943; she and McCulloch had been friends for years before the cybernetics conferences. At the first cybernetics meeting she described the stereotyped, impoverished responses to an inkblot test typical of individuals with organic brain damage, in contrast to the variegated, rich responses of people with intact brains. At the meetings she was often uncomfortable during protracted discussions of mathematical, formal-logical, and engineering topics, because she could not contribute to them. Some participants recalled that they liked having her "feminine presence" at the meetings.[35]

At the second conference Harrower was urged to explain Köhler's "field"; she was reluctant, since she was deeply imbued with Koffka's ideas, which she knew differed from Köhler's. Lewin, known for his friendly demeanor and open-mindedness, found a favorable reception for his own field concept. Lewin was the obvious bridge between Köhler and the cybernetics group, but Bateson anticipated a difficulty, which he conveyed to McCulloch and Lewin:

> I am worried about Köhler, Gestalten, and the rest. It is to me very sad that we have walked into this set of problems via the historical approach and the preliminary attempt to determine what psychologists in the past have meant by "gestalt." However we are now committed to inviting Köhler and he may be an important addition . . . the thing to be avoided is controversy within any one discipline.[36]

Köhler was not part of the Bateson-Mead-Frank tribe. In contrast to Lewin, he had a reputation for a missionary zeal for a Gestaltist philosophy, and response to his lectures was often indifferent.[37] Köhler himself reported Lashley's remark to him, "Excellent work—but don't you have religion up your sleeve?"[38] Bateson made a practical suggestion: let Köhler acquaint himself with the group on the first day and give his presentation on the second. Filmer Northrop also had reservations about devoting conference time to Gestalt psychology. "I am sure that proceeding from the standpoint of physics, mathematics and neurophysiology is much better," he wrote to McCulloch.[39]

One significant difference between Gestalt theorists and Pitts and McCulloch lies in their choice of metaphor. As a matter of principle the Gestalt school opposed mechanical models. Köhler had invented a terminology in which a "machine theory" referred only to the assumption of rigid geometric constraints. Thus he promoted his own field theory as an alternative to a machine theory. Yet, in fact, it merely described a different kind of machine. The machines that interested Pitts and McCulloch, however, were of a still different kind, in that messages, coding, stored programs, and feedback loops played a role.

These three types of mechanism differ as follows. A train running on tracks is a machine whose motion is governed by rigid geometrical constraints. A man-made satellite shot into interplanetary space and then allowed to move freely will be governed by the local gravitational field; if the satellite is sufficiently massive and comes sufficiently close to the moon, its own gravitational field will alter the motion of the moon. This mechanism is analogous to Köhler's "field." But if a satellite is guided by radio control from earth, perhaps sending back information about its position, or if it has a built-in program, then it is a cybernetic machine.[40] Köhler's effort to exempt his model from the category of "machine-theory" reflects the biases of the European culture in which his theory was first developed.[41]

Köhler's ideas about perception also differed from those deriving from communication engineering. As far back as 1920, Köhler had posited a "psychophysical isomorphism," by which "psychological facts and the underlying events in the brain resemble each other in all structural characteristics."[42] At the Macy meetings, however, von Neumann, Wiener, Rosenblueth, Pitts, and McCulloch had used the metaphor of "coding" to characterize that relation. In coding, certain abstract relations are preserved, but concrete structural characteristics need not be.[43] On account of the disparate nature of the two concepts Bateson recommended that coding be one of the major topics on the first day. Lewin and McCulloch agreed and the program was set up accordingly.[44] Lewin's death disrupted all plans and Köhler did not come to the meeting.

The next year Köhler wrote asking McCulloch for help in getting a Macy grant to do neurophysiological studies of electrical potentials in the visual cortex.[45] McCulloch was delighted that Köhler was coming to neurophysiology, and contacted the foundation on his behalf. McCulloch wrote to the skeptical Rosenblueth, "I just had a letter from Köhler who is now working with a group at Princeton measuring the potentials of the visual field under conditions of pattern vision. When the Granddaddy of Gestalt psychology swings back into the physical fold to this extent, I am for the first time happy to think of listening to his notions as to how we see things."[46] Köhler was invited to attend the fourth meeting, and to prepare him, McCulloch sent a long article by Wiener (a precursor of *Cybernetics*) and his and

Pitts's as-yet-unpublished paper describing an automaton/neural net able to discriminate forms.[47] Köhler reported to McCulloch that although he "found a few points of disagreement" with his paper, he did not intend to criticize it at the meeting. "I prefer to say positively how I am trying to correlate psychological facts and neurological possibilities, and for what reason I move in that direction."[48] Everyone, including Köhler, seemed to be working to make his visit a success.

The psychological facts Köhler emphasized were the figural aftereffects studied by the psychologist James J. Gibson, as well as by Köhler and his coworkers. Köhler was convinced that the physiological correlates of those phenomena must be steady electrical currents spreading through the brain tissue, which act as a continuous conductor—very different from discrete pulses propagated from neuron to neuron. Consequently, Köhler conjectured that steady currents play a significant role in the physiology of perception. At Princeton in 1947 he had begun to look for and measure these steady currents, using electrodes attached to the skull of a subject looking at stationary or moving objects or at changing patterns. By 23–24 October, the time of the meeting, Köhler's electrophysiological experiments had not yet progressed very far.[49]

A young experimental psychologist who was to become a regular, Lukas Teuber, was invited as a guest to the fourth conference at Klüver's suggestion (Klüver himself did not attend).[50] Köhler gave his long-anticipated presentation at that meeting, and as expected, it generated controversy. Lorente de Nó did not stay for the second day. Pitts and McCulloch were disappointed because Köhler did not deal with concrete neurophysiological data:

> The real trouble was that Köhler attempted to present a field theory instead of the facts and had, in fact, no theory; for it is the essence of a theory of a field that it prescribes exactly in quantitative terms. . . .[51]

Teuber and Molly Harrower, both admirers of Köhler, were upset. They thought the group had not shown him proper courtesy: What did it matter that Köhler's physics was somewhat simplistic?[52] Teuber recalled: "It was a sad evening and Köhler felt badly that his speech had fallen quite flat."[53] Harrower was dissatisfied with the group and was on the verge of dropping out, but did so only after the next meeting.

Meanwhile, McCulloch persuaded Teuber and Harrower to show the group at the next meeting the phenomena Gestalt psychologists had called attention to, to familiarize the group with the "facts of perception." (McCulloch mentioned that Wiener's and Savage's eyesight was so bad that they would not benefit much.) Typical of many classical Gestalt visual phenomena is the so-called gamma movement, which provides evidence against atomism and for the interaction of local facts: "When an object suddenly appears in the visual field, this object rapidly expands, and when the object suddenly disappears, it contracts. Similarly, when not one object but a group of objects is suddenly shown, the members of this group rapidly move away from each other so that the whole group expands."[54] Another example used as argument against atomism is color vision: "When a gray object surrounded by a white surface is compared with a second object that, physically, has the same gray color but is surrounded by a black surface, the gray-on-white object looks darker than the gray-on-black object. Similar effects of the color of the environment on a local color can also be demonstrated when the surrounding colors in question are so-called hues, that is, red or yellow or green or blue. In a red environment, for instance, a gray object tends to look greenish, and so forth."[55] Another major tenet of Gestalt psychology is the importance of "insight" in problem solving, that is, clearly grasping the relationship of events in a situation. Köhler's observation of apes seeking to get hold of a banana and suddenly discovering that a stick would enable them to do so had been one among many kinds of evidence for the existence of sudden insight in even nonhuman primates. Such insight was taken to have as a concomitant a restructuring of the perceptual field, not a local process. Restructuring can be illustrated by looking at a picture that first seems to be two faces but suddenly appears as a vase. The scientists at the meeting presumably had some acquaintance with a sudden, Aha experience. Particularly germane were the figural after-effects Köhler investigated in the early 1940s.[56] One shows a person some specific form—dots or lines or perhaps a more elaborate shape. After a specified length of time, maybe fifty seconds, that form is removed and another is placed in the person's visual field. Systematic distortions will then appear in the perception of the new form. Straight lines will look curved, objects appear displaced upward, downward, or sideways from their actual positions, distances and sizes will systematically be judged too small or too large, and so forth. These effects have their own rules, most notably that the second form is perceived as displaced away from the area in which the first form had been located, and if the original form had clear boundaries, the displacement is away from those boundaries. To Pitts and McCulloch the demonstration of these phenomena only enlarged the list of what should be derivable from neural net models.

A year after the fourth meeting, the Hixon Symposium in California was convened to permit a more technical level of discussion of the mechanisms in the brain that might correspond to ideas, perceptions, and behavior.[57] Chairman of the symposium was the genial psychiatrist Henry Brosin, a Macy participant. His professional interest was in the nature of genius. Apparently having at some point concluded he was himself not in that league, Brosin was fascinated by the most gifted and brilliant scientists. At the symposium McCulloch and Köhler presented papers describing their work; participants in the discussion included Klüver, Ralph Gerard, von Neumann, Lashley, Lorente de Nó, Donald Lindsley, John Stroud, and Howard Liddell—some of whom were regulars of or visitors to the Macy group. By the time of the symposium Köhler had more data than a year earlier, and he marshalled his facts and arguments. The neurophysiological counterpart of the figural after-effect, Köhler insisted, had to be a direct current that polarizes the surfaces of the cells as it passes through neural tissue, and the polarization will in turn weaken the current and deflect it from its initial distribution. Moreover, Köhler said, the presence of the current will increase the polarizability of the cell surfaces, thus intensifying the effect. All this supported the belief in a continuous electrophysiological field in the brain as the physical correlate of perception.

McCulloch described the Pitts-McCulloch neural net model, paying particular attention to Lorente de Nó's data on the properties of individual neurons, the role of circuits with negative feedbacks, and his own and Pitts's model of the mechanism (and its anatomical substrate) for perceiving forms. In their model the transmission of a discrete pulse from one neuron to its neighbor was the elemental event, the "atom" out of which thought and perception were constructed.

Participants at the Hixon Symposium credited both Köhler's and McCulloch's views with some validity. As Lashley put it, "Dr. McCulloch has been inclined to regard the phenomena dealt with in field theory as smear effects to be disregarded in the development of our concepts of neurological function. Dr. Köhler, on the other hand, has minimized the importance of neuronal discharges in the integrative functions of the cortex. I hope that we can find some way of bringing these two points of view together, because I feel that both deal with genuine phenomena which are significant for understanding behavior."[58] But the field theory seemed at a dead-end and, said Lashley. "I am at a loss to see where further developments of the theory will lead."[59] Lashley's prognosis about the neural net models, however, was "that any understanding of the nervous system we may acquire must be developed within the framework of our knowledge of the activities of the individual neuron. There may be additional factors introduced by combinations of which we know little or nothing at present, but the general principles seem to me to be fundamentally correct. At the present time, however, such a formulation involves a very great oversimplification of the problem."[60] In fact Lashley reported some neurosurgical studies of monkeys that seemed to refute the specific anatomical localization of the perception of forms suggested by Pitts and McCulloch. Lorente de Nó said of the neural net models,"I think that probably many of the details will not stand, but the main concept will certainly remain."[61] Von Neumann, notwithstanding his characteristic scientific optimism, noted the limits of the Pitts-McCulloch model for recognizing forms, and suggested that for the general problem of visual analogy "the order of complexity is out of all proportion to anything we have ever known. . . . It is, therefore, not at all unlikely that it is futile to look for a precise logical concept, that is, for a precise verbal description, of 'visual analogy.' It is possible that the connection pattern of the visual brain itself is the simplest logical expression or definition of this principle."[62] Two years later at the seventh Macy meeting, Klüver viewed both Köhler's and McCulloch's models with skepticism: "I do not know how the factors governing the appearance or disintegration of even simple visual *Gestalten* are related to analogical or digital functioning or to what extent, if any, an experimental analysis of such factors may benefit by digital or analogical models."[63] As to the neurophysiology, Klüver was reaching for a different approach: "It may be argued that the eye and the subcortical and cortical structures of the visual sector of the central nervous system represent more than merely delightful opportunities for anatomical and electrophysiological researches. I may be forgiven for mentioning the old-fashioned but nowadays apparently somewhat radical idea that these structures have something to do with *seeing*."[64] Perception of form was still not understood.

Jerome Bruner (quoted in the epigraph to this chapter) has written of the "almost tragically quixotic quality" of Köhler's experiments recording direct currents in the brain. And McCulloch has been described as "stubbornly . . . tilting against the epistemological windmill."[65] In a sense, both were romantics, Köhler in his holism and McCulloch in his mechanism. The Don Quixote label is applicable in retrospect if one agrees with J. J. Gibson that both approaches were heroically pursuing wrong, outdated questions. Both offered simplistic answers, however scientific, to an immensely difficult problem, as von Neumann most clearly appreciated. Köhler's and McCulloch's efforts to reduce their deep differences in outlook to tests in the neurophysiological laboratory is testimony to their commitment to science.

At the tenth and final Macy meeting McCulloch reported on the status of his and Pitts's theory of how we recognize shapes and musical chords. He reported Lashley's strong arguments against the specific mechanism they had proposed. Moreover, Donald McKay, a young Briton whom McCulloch had brought to the eighth meeting, had tested the role of scanning in McCulloch's laboratory and the result again refuted the detailed mechanism. McCulloch cheerfully concluded from this that we can invent mechanisms, make hypotheses, and disprove them. Thus we are right to regard our work as scientific epistemology.

But the new paradigm for cognitive psychology—mathematical and computer modeling joined with neurophysiological research—was an exciting one. It suggested scores of research programs, mathematical modeling, hardware simulacra, and neurological investigations. What are the codes? What is memory for a robot and for a human? How might ethical notions be embodied in a nervous system or a robot? How would one construct a machine to recognize some of the patterns a human recognizes, or, more generally, to carry out some class of functions characteristic of human mental activities? What, if any, role does "scanning," a familiar mechanism in television, play in neural nets? What localization occurs in the central nervous system? The new paradigm was irresistible not because field theories had been refuted, but because it posed such inviting research problems. The needed technology was on the horizon. Pitts and McCulloch knew, especially as Gerard had repeatedly emphasized it at the Macy meetings, that continuous electrical potentials and currents can spread through brain tissue, but they dealt with that by suggesting that statistical averaging of impulses takes place in the brain, smoothing out the atomistic character of individual synaptic firings.[66]

A few years after his visit to the Macy group, in a review of Wiener's *Cybernetics,* Köhler restated his belief in field theory and made a pithy observation about what seemed to him an essential limitation of computer models:

> As to the now popular comparisons between thought and the operations of calculating machines, the greatest reserve seems to be indicated. . . . The machines do not know, because among their functions there is none that can be compared with *insight into the meaning* of a problem.[67]

He continued with the comment that "when the mathematician or the engineer begins to deal quite freely with problems of psychology," they tend not to take cognizance of the "actual knowledge of modern psychology." He did acknowledge, however, the usefulness of the feedback concept in the study of perception.

Some years later Jerome Lettvin, the one member of the McCulloch-Pitts-Lettvin team who was not in the Macy group, carried the study of vision forward by a major step reported in a paper he published with Pitts, McCulloch, and Maturana, "What the Frog's Eye Tells the Frog's Brain."[68] Lettvin did the experiment, but he discussed it and its interpretation with Pitts and McCulloch. He identified four different kinds of nerve fibers connecting the frog's eyes to the brain. Putting microelectrodes into the optic nerve of the frog and presenting the frog with various visual stimuli, he demonstrated that each of the four types of optic nerve fibers of the frog has a particular function, each responding with electrical activity to a particular class of patterns of visual stimuli and resulting in a particular type of action by the frog. Thus he linked the visual field directly to anatomically defined electrical activity and to behavior, albeit only for a frog. For example, one type of fiber responds only to moving objects in the visual field that are small, dark, and have a convex boundary. The response is stronger if the object's motion is jerky rather than smooth. These fibers, Lettvin and coworkers suggest, could be interpreted as "bug perceivers," or characterized, more formally, as convexity detectors. They concluded that the terms in which the observations of the frog "are best described is the language of complex abstraction from the visual image." Pitts and Lettvin regarded these results as an exemplification and corroboration of Kant's idea of the synthetic *a priori*. The experiment also confirmed the concept of coding and was in clear conflict with Köhler's "psychophysical parallelism," even as it called attention to configurations—*Gestalten*—in perception rather than points of light. These frog experiments encouraged researchers in their search for details of the process of perception that might, after all, become scientifically comprehensible.

Some of the continuity in the history of psychology, from Gestalt or behaviorist psychology to psychology in the neomechanists' and information theorists' idiom, is revealed in the memoirs of Jerome Bruner, a psychologist then in his thirties. Köhler had been one of his heroes, and Bruner decided to look him up:

> I visited in Swarthmore, the New World "home" of Gestalt psychology, presided over by the distinguished and aristocratic Köhler and his assistant Hans Wallach. . . . Köhler's singlemindedness expressed itself in the search for a neural "isomorph" to the figure-ground phenomenon in vision—the heart of the Gestalt metaphor. That is to say, if one found a phenomenologically "simple" *visual* form, then one should expect to find a counterpart neural process that corresponded to it, that was somehow *similar* to it topologically or even geometrically. Köhler was recording direct current in the brains of his subjects while they viewed sharp-contoured figures passing across the visual field. It was a gallant search for a geometric neural analogue of an experienced figure on a ground. Looked at historically, looking back to the early 1950s, it has an almost tragically quixotic quality. For it was not long after that Hubel and Wiesel were to find that the visual system of the brain operated as a coding system, particular receptors firing in response to visual slant, to edge, to contour, etc., the whole of it then getting put together at some upstream editorial center. (These results which were obtained with a cat as a subject confirmed the simultaneous results of Lettvin et al. on a frog). . . . Hubel and Wiesel were awarded the Nobel Prize for Medicine for this very work.[69]

Bruner was at the time teaching in the Social Relations Department at Harvard, set up by Talcott Parson, Clyde Kluckhohn, and others a few years earlier. But he was not imbued with the personality and culture theme, "thinking instead in terms of coding systems, the flow of information through them, the heuristics, and the clever ways in which that information was combined and recombined in the service of coding."[70] He had learned about information theory. He and colleague George Miller called their approach to psychology the New Look. It was based on metaphors from the neomechanists, although not the specific Pitts-McCulloch model, and in retrospect gives "thanks, ironically, to the liberating effect of the computer on the psychologist's image of what is humanly possible."[71] In 1960 Bruner left the Social Relations Department and with George Miller set up the Center for Cognitive Studies at Harvard, where their lines of work could be expanded. Perhaps because it seemed too vague, even mystical, lacked clear models of process and an intriguing research program, Gestalt psychology had been relatively ineffective in challenging the dominance of behaviorism in the United States. The cognitive studies program presented more explicit mechanistic models, offered new research topics, and, for some, provided a convincing challenge to narrow behaviorism. It found particular favor in schools of education in the United States.

Continuity from McCulloch's (and Pitts's) thinking was provided by a large number of his younger associates and friends. Jerome Lettvin, Marvin Minsky, Seymour Papert, and Oliver Selfridge are well-known names among those who explored perception or cognitive processes further. This is not the place to describe the development of the psychology and epistemology of perception since the Macy conferences.[72] It is important, however, that since 1980 a new class of neural net models in the spirit of Pitts and McCulloch have been devised and elaborated under the name of "massive parallel processing" or "neoconnectionism," and their study and refinement is an active area of research.[73] They appear to model various cognitive functions quite naturally and are linked to neurobiological studies.

At present one may distinguish two contending types of approaches to understanding perception. The first is within the general framework of what has come to be called cognitive science. The other is that developed by James Gibson in particular. The ground of the controversy has shifted from that between McCulloch and Köhler. In their outlook the cognitive scientists are closer to Pitts and McCulloch, and the Gibsonians—in spite of major differences—have more in common with the Gestalt theorists. The relative popularity of the cognitive science approach, while partly due to intrinsic scientific interest, is enhanced by fascination with artificial intelligence and computer technologies and strong financial support from the Department of Defense and industry.[74] The following descriptions illuminate the contrast in the suppositions of the two contending views.

Cognitive scientist Allen Newell, defining "cognitive science," says:

> Its underlying proposition is that theories of human voluntary behavior are to be sought in the realm of *information-processing systems*. This is to be understood in the same sense that theories of macrophysics are to be sought in the realm of *differential equation systems*. There is a type of system—called an "information-processing system"—that consists of *memories* and *processors* (also *transducers, switches, controls, data operations,* and *links,* to be complete). The system works on an internal medium of *data structures,* which represent things and situations. It performs operations on these representations to compute new representations and thereby generally manage its affairs in the world. . . . The central agreement is that a human is that kind of a system, it being open and the object of empirical investigation to find out the particulars.[75]

Howard Gardner considers the following as the central beliefs in the practice of cognitive science:

> 1. "In talking about human cognitive activities, it is necessary to speak about mental representations and to posit a level of analysis wholly separate from the biological or neurological, on the one hand, and the sociological or cultural, on the other." 2. "Central to any understanding of the human mind is the electronic computer . . . as the most viable model of how the human mind functions."[76]

He adds that, as a matter of strategy, the influence of affective factors or emotions, the contribution of historical and cultural factors, and the role of the background context in which particular actions or thoughts occur, should be ignored or at least de-emphasized. Furthermore, cognitive science must be interdisciplinary and concern itself with traditional issues of epistemology. However ingenious the robots designed by artificial intelligence researchers and sophisticated the formalisms describing parallel distributed processing in neural nets, the simplifying assumptions of the cognitive scientists are so extreme that one might reasonably suspect they are throwing out the baby with the bath water. Moreover, attempts to test models in the laboratory tend to yield only inconclusive results because of what Lettvin has described as "the physical intractability of nervous tissue."[77]

Gibson's premises and procedures, more naturalistic, philosophically realistic, and pragmatic in William James's sense, but not formal-logical, are a sharp contrast.[78] At the Macy meetings Klüver had called attention to areas of ignorance in the psychology of perception. Gibson challenges traditional approaches to perception and is concerned to begin by posing the right questions, because, he wrote, "the conclusions that can be reached from a century of perception are insignificant. . . . A fresh start has to be made on the problem of perception."[79] In 1966, after expressing his astonishment at the unwarranted self-confidence of psychologists, Gibson commented that scientific progress in psychology seemed puny and scientific psychology ill-founded. "At any time the whole psychological applecart might be upset."[80] According to Gibson the whole tradition is obscurantist. It fails to evaluate properly the distinction between sensation (passively receiving stimuli) and perception (actively obtaining information, exploring, and selecting). The cognitive scientists in particular perpetuate this obscurantism. The organism is not a machine processing all inputs from the environment, but an active, selective creature that needs and wants. The interpretation of Lettvin's frog experiments had implied that a frog's perception is biologically rigidly structured by its needs, but Gibson would have given the experiments a realistic interpretation rather than Lettvin's and Pitts's Kantian one. The notions of information and feedback loops play an important role in Gibson's views, but the formal-logical computer model does not. The environment, the context, and the biology, however, are all taken seriously; in fact, the environment-organism relationship is regarded as one of mutuality. A traditional question, for example, is the invariance of our perception of the world around us in the face of constantly changing sensations. Pitts and McCulloch had hypothesized, at least in the case of a triangle or a musical chord, that the object perceived is identified by undergoing numerous mathematical transformations in the automaton (logical structure, software) associated with the human brain. Gibson explains recognition of a table as a table in terms of the observer's physical motion in relation to the table and the attendant transformation of sense impressions. Gibson views perception as an active process, and it is his hypothesis that "constant perception depends on the ability of the individual to detect the invariants, and that he ordinarily pays no attention whatever to the flux of changing sensations."[81] We move our heads or eyes to seek the information we need or want from the environment: "Instead of supposing that the brain constructs or computes the objective information from a kaleidoscopic inflow of sensations, we may suppose that the orienting of the organs of perception is governed by the brain so that the whole system of input and output resonates to the external information. . . . The classical concept of a sense organ is of a passive receiver, and it is called a receptor. But the eyes, nose, mouth, and skin are in fact mobile, exploratory, orienting."[82]

Clearly, the Gibsonians and the cognitive scientists have conflicting premises concerning what essentially constitutes a human being, and they also differ radically in their assessment of how adequately modern science understands perception.

Although the cognitive science approach may get larger financial backing than the Gibsonian because of extrascientific interest in artificial intelligence, the intensity of the controversy and the high level of research activity suggest that the psychology of perception is alive and well.

11

Metaphor and Synthesis

The set of ideas McCulloch, Pitts, von Neumann, Wiener, Rosenblueth, and Lorente de Nó brought to the Macy meetings could be put to many kinds of uses. They served well as metaphors for representing substantive chunks of the world we know. It is not unusual for the concepts used in a scientific theory to be extended beyond its strictly technical domain by means of metaphor so as to try to generate a comprehensive and coherent world view: It happened to Darwin's theory of evolution, to psychoanalytic theory, to thermodynamics, to Marx's economics. In no case have the set of concepts from such a theory been sufficient to give a total synthesis of our experience. Invariably they highlight some aspect by emphasizing particular interpretations. Other elements of reality are either ignored or brought in to supplement whatever the set of concepts could encompass. The resulting synthesis would then be unique to its particular inventor, his or her way of putting it all together. Because of the degree of arbitrariness in such a synthesis, it would never win the assent of scientists generally.

Several of the participants at the Macy conferences were passionately interested in putting it all together. In an eclectic style they forewent the austerity of positivists. They stuck to the "important" questions even if they could not be neatly settled in scientific terms alone. They understood that the western scientific view of the world was not the only one, and that believing in it entailed a choice. The four synthesizers—Wiener, Bateson, Stroud, and Northrop—each created an individual view of things. They represent, for the historian, a significant sample from the wide spectrum of syntheses constructed by Macy participants. Each in his own way was much devoted to the theories and practices of modern science.

Wiener's synthesis is extraordinary not only because of his first-hand acquaintance with so much of science and technology, but also because of his use of the language of cybernetics to examine a wide range of social and political issues from a humane standpoint.[1] He emphasized the comprehensiveness of cybernetics as the theory of the message among people, machines, and in society. Messages, in Wiener's view, not only involved contingency but were centrally important in the context of purposes to counter nature's tendency to disorder. In his intellectual synthesis Wiener sought to demonstrate that the concepts of cybernetics form a fundamental unity with Augustinian theology on the one hand and the physics of Newton, as extended by Gibbs, on the other. The predominant idiom, nevertheless, of his unification was that of communication engineering. Actually, Wiener took the relation between engineering devices and the description of people in many applications to be stronger than mere metaphor. He and the other Macy participants spoke of the "functional equivalence," for example, between computers and parts of the human brain, and of theory intended to encompass humans and engineering devices equally. Although Wiener discussed social and political issues using metaphors from cybernetics, for that purpose he found it useful to bring in ideas from outside of engineering, such as, for example, liberty, equality, and fraternity.

Bateson's synthesis is more a manner of thought and a direction for further elaboration than it is a closed, complete system. It has been viable and has inspired a following. He took the concepts of cybernetics and made them serviceable, first for the social sciences, and later as part of a practical epistemology useful outside of social science.

In 1946, after the first Macy meeting, Bateson wrote a remarkable brief position paper to identify scientifically "legitimate and rewarding" procedures using analogy "to argue from physical mechanisms to organisms and from organisms to society," and also to note the "dangers inherent in this type of procedure". He wrote,

> We accept the statement of mathematicians and physicists that when the relevant variables in a system are formally interrelated in certain ways, the system will have certain properties. Specifically . . . when the system of variables is circular or reticulate, a formal understanding of other such systems, whether derived from abstract mathematical ar-

> gument or actual experience in mechanics, biology, or social science, is certain to help us in tackling our special problem whatever the field in which we are working. . .
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> Those who use these analogies to construct hypotheses must bear the onus of proving that the variables with which they deal have in fact the formal interrelations specified in the hypothesis. Those proofs will consist partly in tracing the variables and their interrelations all around the causal circuits, and partly in making verifying predictions about the properties of the system. Conversely, when we observe in the social sciences or in biology that the system with which we deal shows such properties as have been described for circular or reticulate causal systems—properties of self-correction, self-maximization, oscillations, etc.—it is legitimate and likely to be rewarding to construct some hypotheses postulating that the system is in fact circular or reticulate. . . .
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> The properties of circular and reticulate causal systems have only recently been brought effectively to our attention, and our immediate task is one of planning. We need to survey the knowledge which we have in order to re-orient ourselves to the profound changes in the conceptual landscape which these new ideas bring with them. We need to plan the new questions which we must now ask and to envisage the new types of data which will be necessary for answering these new types of questions.[2]

As a first step, Bateson proposed a classification of phenomena, distinguishing those that show characteristics typical of circular causal or reticulate systems (he mentions business cycles, armaments races, schismogenesis, phenomena of segmentation of society) from the category where one has some knowledge of reticulate or causal circuits from the outset (e.g., systems of checks and balances in government). For the first type of phenomena the task is to search out actual causal paths and make the initial hypothesis detailed and precise. For the second category the task is to make predictions about the properties of the system and to test them. A criterion for further classification distinguishes whether the system is contained within the body of a single individual, or whether the causal arcs pass through two or more individuals or through the organized structure of society. An additional criterion is the order of complexity of the pattern of circuits. Bateson concludes with the comment that, "in the social sciences we need not less but rather more rigorous thinking than is usual among the physicists [*sic*].[3] The entities with which we deal are much more complex than even their computing machines, and by that token we need more complex, more flexible, and more precise conceptual tools."[4] He was clearly proposing a bold and far-reaching research program. He did not yet mention the analogy with Russelian paradoxes which became centrally important for him later.

To Bateson's mind, cybernetics offered a new organizing principle in the social sciences, as had the theory of evolution for biology. In terms of traditional academic disciplines his work from 1946 on seems to jump from one field to another: from anthropology and learning theory he moved to psychiatry, behavior of otters and octopus, theory of humor, kinesics, language and learning among dolphins, and theory of evolution. In fact in each of these research activities he was working out in specific situations the program he had enunciated in 1946.

His activities became increasingly marginal to the academic mainstream and its institutions, aside from his role of "scout." In California he was centered at the Veterans Administration Hospital, made forays to the San Francisco Zoo to study otters, kept in touch with Wiener, McCulloch, and Hutchinson, attended Macy meetings, and talked with psychiatrists and students of Zen Buddhism (especially Alan Watts). Fremont-Smith, or rather the Macy Foundation, helped to fund his projects. Although he lectured at Stanford University he was peripheral to the institution, just as it was peripheral to his world. He moved to Hawaii in the 1960s to become part of the research arm of Sea Life Park, a public ocean aquarium, geographically and intellectually still further removed from the mainstream. By that time he was no longer an active member of the tribe to which Frank and Mead continued to belong. He was going his own way.

I looked up Bateson in Sea Life Park in 1968. I had written to him in connection with my interest in the history of the Macy cybernetics meetings. His response was, "There is certainly a piece of scientific history to be dug out of these meetings—I believe more profound and dramatic than *The Double Helix*."[5] In his opinion the importance of reductionist molecular biology was overrated. The impression he left me with is complex and many-faceted, but it was that of an essentially gentle man, whose relation to otters and porpoises was that of a scientist-playmate-observer with an active appreciation and respect for life.

He was at times interested in political problems and applied his understanding of anthropology, cybernetics, and language to international dangers, although on the whole he tended to dismiss politics and economics as the branch rather than root. One such occasion arose in 1962, when Bateson's project in California had come to an end and he was exploring new directions for research. He had a way of looking at how to communicate nonviolent intentions that might be pertinent to political confrontations. By means of behavior, "the transmitter of the message must in some sense mention violence . . . and must somehow introduce a negative into this analogic statement about violence."[6] This was the time of the confrontation between President Kennedy and Premier Khrushchev over Russian missiles in Cuba. Bateson thought, "the great danger at the moment is that Khrushchev's willingness to negotiate may be taken by us as fear." Bateson's notion had been stimulated by his watching octopuses:

> The negatives can only be communicated by total sequences of interchange in which these negatives are exemplified. The octopuses, starting from mutual hostility, pass through a sequence of minor battles in which nobody gets hurt much. After this the slightly stronger octopus very slowly and gently embraces the weaker, i.e. states, "I can hurt you but I am not doing so." Following this, the weaker comes over and attacks the stronger with his vulnerable backside, in response to which the stronger retreats. I.e., the weaker has now said, "Yes, I know you are not going to attack me" and the stronger has said, "That's right."[7]

Bateson discussed these ideas with McCulloch, who passed them on to Jerome Wiesner. Wiesner had attended one of the Macy meetings as guest, but in 1962 was President Kennedy's science advisor. Eventually Bateson received a reply to the effect that the Arms Control and Disarmament Agency would welcome a specific research proposal, submitted through channels, elaborating Bateson's idea.[8] Meanwhile Bateson had found a different and more congenial new direction, working with dolphins to attempt to understand their language. But in 1969 he was clearly troubled about the future.

> Perhaps we have an even chance of getting through the next twenty years with no disaster more serious than the mere destruction of a nation or group of nations. I believe that this massive aggregation of threats to man and his ecological systems arises out of errors in our

habits of thought at deep and partly unconscious levels. . . . There are patches of sanity still surviving in the world. Much of Oriental philosophy is more sane than anything the West has produced, and some of the inarticulate efforts of our own young people are more sane than the conventions of the establishment.[9]

In 1972 Bateson presented his ideas to the larger public by publishing an anthology of his work. Although the anthology is subdivided into sections, such as Form and Pattern in Anthropology, Form and Pathology in Relationship, Biology and Evolution, to make it accessible to specialists, Bateson's commentary on the papers makes clear that his work is a unity, and that through it he is "proposing a new way of thinking about ideas and about those aggregates of ideas which I call 'minds'."[10] The notion of analogy linking different realms helps to give coherence in a manner very different from reductionist science: "The man who studies the arrangement of leaves and branches in the growth of a flowering plant may note an analogy between the formal relations between stems, leaves and buds, and the formal relations that obtain between different sorts of words in a sentence."[11]

The favorable reception of the book led to his increased participation in conferences and seminars in the United States and abroad. At a meeting of scholars and scientists in 1975 Bateson spoke about the development of an epistemology revealing the unity of mind and body. He was in his seventies by then, and the need to display the coherence of his knowledge, and to address plainly the relation of mind to biological evolution was uppermost. His next book, *Mind and Nature—A Necessary Unity,* was such an effort at synthesis and exposition; the final stages were done with particular urgency, and with his daughter's help, since a malignant tumor had been discovered in his left lung. At this stage of his life Bateson functioned not only as a "scout" but could on occasion be likened to a "shaman."[12]

Although *Mind and Nature* was intended as a popularized summation of the earlier anthology, it reached back more emphatically to the profound issues surrounding mind, materialism, and theories of evolution, issues that had been alive in Bateson from childhood—and that continue to be the subject of controversy. His scientific objective in the book was to "reexamine the theories of biological evolution in the light of cybernetics and information theory."[13] The exposition is preceded by a rather pedagogical discussion of diverse elementary notions (entropy, sacrament, syntax, number, the pattern that connects) that he considered indispensable. They were not widely understood because, to quote Bateson, "schooling in this country and in England and, I suppose the entire Occident carefully avoids all crucial issues."[14] (From 1974 on Bateson was a part-time lecturer teaching undergraduates according to his own lights in an experimental interdisciplinary program at the University of California in Santa Cruz. Lack of funds had terminated the dolphin research on Oahu.)

It would be futile to attempt a succinct summary of the book, which is Bateson's own summing up near the end of his life. He conceptualized it thus:

> Both genetic change and the process called learning (including the somatic changes induced by habit and environment) are stochastic processes. In each case there is a stream of events that is random in certain aspects and in each case there is a nonrandom selective process which cause certain of the random components to "survive" longer than others. Without the random, there can be no new thing. . . . We face, then, two great stochastic systems that are partly in interaction and partly isolated from each other. One system is within the individual and is called learning; the other is immanent in heredity and in populations and is called evolution. One is a matter of the single lifetime; the other is a matter of multiple generations of many individuals. The task is to show how these two stochastic systems, working at different levels of logical typing, fit together into a single ongoing biosphere that could not endure if either somatic or genetic change were fundamentally different from what it is.[15]

To describe these processes and their interplay more concretely, Bateson invokes a hierarchy (logical typing), codes, information transfer, analogical and digital signals, self-correction, feedback loops, response to differences, circular causal processes, and other concepts from cybernetics.

In all, Bateson's synthesis must be seen as a personal one, a lucid image of the world he found congenial. At the same time it adumbrates a research program; in various respects others have found his approach to the biological and the social plausible or even cogent. And yet his procedure of using metaphors from logic or engineering in the social sciences, where he perceived analogous formal relationships, was only rarely amenable to testing in the way one would like for a scientific hypothesis.

Bateson was a remarkable figure. He insisted on addressing questions he deemed important, whether scientific methods were available to provide answers or not. He went his own way, sacrificing the comfort provided by lifelong association with an academic or research institution or a particular profession. By the 1960s he had long left the tribe centered around Holderness, New Hampshire. Except for relatively brief periods, however, he always managed to find financial backing and institutional settings where he could pursue what interested him at the time, however idiosyncratic his questions or iconoclastic his approach. His vitality, intellectual and human, remained intact up to the time of his death, and in the end he was—however belatedly—widely heard and recognized.

By the 1960s McCulloch was no longer part of any influential scientific tribe comparable to the early cyberneticians. But with his perennial spirit of adventure he had pursued the inquiry that intrigued him most: mind and brain. Their interrelation was the nub of his studies, but medieval scholarship, Shakespeare's sonnets, the formal-logical writings of Charles Saunders Peirce, the triadic logic of the Stoics and Darwinian evolution all were elements in his continually evolving personal synthesis.

Bateson and McCulloch had both consciously broken away from academic strait jackets. When in 1968 Bateson organized a conference in Austria, Effects of Conscious Purpose on Human Adaptation, he was especially happy that the frail and aging McCulloch would be a participant. It would be a kind of follow-up on the 1946–1953 cybernetics conferences, but, except for Bateson and McCulloch, with a whole new set and younger generation of participants. Among them was the anthropologist Mary Catherine Bateson, who had spent her childhood summers in Holderness in the world of Mead-Bateson-Frank. Gregory Bateson and Warren McCulloch had by that time gone their separate ways, independent of elite "units of microevolution," and both were equally imbued with the spirit of cybernetics. Both were able to roam widely in their thinking about everything that mattered, and McCulloch especially forewent conventional inhibitions. Mary Catherine described a particular part of the group discussion as "an enchanted time when the comments of different people dovetailed with an intoxicating felicity."[16] Such a description would not have fit any substantial portion of the more narrowly scientific original cybernetics conferences. This extract catches the flavor of McCulloch during this "felicitous" conversation:

> Warren was speaking very slowly. "I am by nature a warrior and wars don't make any sense anymore. I am a king, but I'm an anarchist, and in my country, there are simply no laws, not even this one. I went to work modeling the structure of water in tendon. We went out and bought a bunch of jellybeans and toothpicks and modeled the water. And when I found that I had to write on the blackboard, 'the jellybeans are not to be eaten,' and it wasn't until then that I understood why the Pythagorians had a law against eating beans. Now, the difficulty is that we, who are not single-cell organisms, cannot simply divide and pass on our programs. We have to couple and there is behind this a second requirement." Warren began to weep. "We learn . . . that there's a utility in death because . . . the world goes on changing and we can't keep up with it. If I have any disciples, you can say this of every one of them, they think for themselves."
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> Very softly Gregory said, "Sure, Warren."
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> "Freedom from and freedom for." We sat in silence for a long pause. "Coffee?" said Warren.[17]

John Stroud, who also used cybernetics to give form to his personal metaphysics and synthesis, was a lively, active participant at the Macy meetings. Everyone at the conferences agreed he was a bright and talented young scientist, but a quarter-century after the meetings several participants asked me, "Whatever became of John Stroud?" Except for his presentations at the Macy meetings, his remarks at the Hixon symposium in 1948, and an article he wrote based on his Ph.D. dissertation (he never obtained the degree because of a quarrel with Stanford University psychology professors), he is unknown in the world of science. He is something of a mystery figure. His story and ideas are not likely to be recorded anywhere except in a historical study of the Macy conferences, although Bateson, Mead, McCulloch, and others of the Macy group refer to his ideas in some of their publications, and the work described in his one article found its way into some books on cognitive psychology.[18] So it is appropriate to give him space here. A McCulloch protégé, Stroud was somewhat at loose ends in the late 1940s. He decided after the Stanford debacle to become part of the Civil Service as a scientist, judging this would afford the opportunity to earn a living and enjoy the freedom to pursue his cross-disciplinary interests. He spent most of the rest of his working life as a civilian employee of the U.S. Navy, his work hidden from the scientific community by the veil of military secrecy.

Stroud was born in 1913, the son of school teachers, and grew up in small villages in western Kansas. Later, in England, he joined the Royal Air Force and worked as an electronics technician. He was impressed by the writings of Oswald Spengler and Arnold Toynbee, and also read Pitirim Sorokin's and Lewis Mumford's major works. He became fascinated with the notion of a sequence of civilizations, their rise and decline—the Roman civilization, then the Christian, and now the mixture of Christian and scientific civilizations. Of himself he said, "My cultural heritage is that of the frontier."[19] He saw himself in the vanguard helping to champion and implement the next step in cultural evolution: transition to a genuinely and fully scientific civilization. Faith in that direction of progress made his life and work meaningful as participation in a large-scale drama. Whereas nowadays only some people make decisions on a scientific basis, and that only some of the time because scientific civilization is still immature, in the utopian future Stroud envisioned decisions would be made on a consistently scientific basis. His concept of science incorporated "ethics," and he sometimes regarded those scientists who claim their work is ethically neutral, or who accommodate the professional requirement of narrow specialization, as disingenuous and cowardly. While most scientists fell far short of his ideal, the members of the cybernetics group and the Hixon symposium came closer in his judgment than most, and he was inspired by being among them.

In England Stroud had made the acquaintance of the Cambridge University psychologist Frederic Bartlett, and had become interested in psychology, especially the work of Kenneth Craik. During the war Craik had concerned himself with a human being situated between an information output and a gun he must aim and fire in accordance with information received continuously. Craik asked, "What kind of machine have we placed in the middle?," and came to the conclusion that the human operator was an intermittent servo. Craik, and Stroud after him, found it a useful working hypothesis to view humans as machines. To quote Craik:

> The interesting thing in perception, surely, is not just what happens, but how and why it happens, and what has failed in the case of illusion

or insanity. To go no further than the "forces" (of the Gestalt psychologists) is like explaining a railway collision by saying that two trains were drawn together by a force. It is more fruitful to investigate the mechanism. Then, perhaps, we shall find that a brake failed or a signal jammed.[20]

Warren McCulloch also admired Craik's work and his clear mechanistic assumptions, and helped to bring them to the attention of American readers.[21]

Stroud's Stanford psychology experiments, carried out in the physics department laboratories, were to identify an elementary unit of time, "the psychological moment." He noted, as William James had done before him, "that the 'now' of psychological experience was not infinitesimally short, was in fact so broad that it quite routinely made physically non-simultaneous events psychologically simultaneous."[22] For example, if on an oscilloscope a pip moves from right to left, but moves so fast as to occur all in one "frame," the observer sees a streak and cannot tell whether it moved from left to right or vice versa. Stroud studied the connection between his psychologically discrete time unit and the continuous time of conventional physics. The discreteness is not too surprising, for neuronal processes of response to a visual or auditory impulse take time, and each neuron has a refractory period. Stroud surveyed all available experiments bearing on the subject and performed a variety of new ones, some using light and others sound, with the relatively high precision electronic instrumentation available in 1950. The upshot of his systematic analysis is that the duration associated with a "moment" always lies between 1/5 and 1/20 of a second. Two incidental comments from Stroud's article show something of his nature. His article begins: "Man is the most generally available general purpose computing device. As such he plays many different roles in complex systems, transforming some time varying inputs into time varying outputs." In naming the discrete time unit, he wrote: "The word 'moment' was chosen because poets had sometimes used the word in a very similar sense of the least possible timewise element of experience."[23]

I never met John Stroud, but I wrote to him in 1972 asking for his recollections of the Macy meetings. In reply I received not a letter but a cassette, because Stroud preferred oral to written communication, and he asked me to reply in kind. In the ensuing years a casette correspondence developed, and I received about a dozen audiocasettes in which Stroud described the Macy conferences, his life and diverse thoughts and interests. Of himself he said, "I have never succeeded in finding anything practical to do with my thoughts—as far as the other fellow is concerned—I may have lived my life solely for the purpose of satisfying my own curiosity. With the exception of some informal discussion (such as we are conducting via tape) I have never found any market for these notions." Perhaps this was because of his disinclination to write and publish, the military (secret) context of his work, and his working at an institution without students; possibly his ideas did not commend themselves to others. The notions to which he referred were primarily those of his unifying theme, not the details of his technical work.

In his twenties Stroud wrote a long poem that identified a perceived unity; the poem grew out of a turbulent personal experience, and this experience was one of the sources for his later systematic, logically formulated views. Cultures come and go, he acknowledged in the poem, but life continues. He was not speaking of the life of an individual organism, for "the living thing you see that is not life—that is but a bit of universe brought within the forms of life/ . . . these beings come and go, momentary foci in the form/ and so with mice . . . man may at last learn to see the grandeur of all life of which he is a part."[24]

Portions of the poem suggest Nietzsche, Comte, mysticism, technocracy, pantheism, but the poem as a whole defies categorization: It was his private vision at age twenty-five. The perception of the earth and its biosphere as a living entity of which plants and animals are subsystems has, with the help of concepts from cybernetics, been put forth recently in a new form, buttressed by scientific data gathered from other planets, as the Gaia hypothesis.[25]

The events of Hiroshima and Nagasaki got Stroud interested in understanding the requirements of a human habitat. In view of the cycles and sequences of human civilizations, how can life be assured continuity in case of nuclear war? Stroud, in a utopian vein, saw a possibility in colonizing space—many small city-states spread over the interior of the solar system out to the orbit of Jupiter—so that a nuclear war on earth would harm only the unadventurous who had remained behind. He designed tin-can-shaped units for interplanetary human habitation, each the locus for one city-state. He considered the process of obtaining materials for construction from meteorites or asteroids, and worked out many construction and design details. Some of his thought and calculation found its way into a Navy report, which was promptly stamped SECRET. Stroud told me in a cassette-letter:

> I just wanted to find out if there were any kind of solution that was possible, that essentially depended on purely physical and biophysical means which could, as it were, guarantee the continued existence and evolution and development of humanity, quite independently of whatever kind of mess we made of the earth's surface.

The space colonies were to be a kind of Noah's Ark. The earth would become a Williamsburg—a place to visit, but of historical interest only. He told others at the cybernetics meetings of his ideas. Margaret Mead tried to get him to publish the work and offered editorial help. She believed that it presented another kind of option, which should be part of the dialogue. A publisher was interested, but was unwilling to let Stroud place it in the context of his ethics and philosophy of history. Since to Stroud the ethical motivations and historical considerations were part and parcel of the idea, it was never published. He had no talent as a publicist for his own ideas. More than twenty years later physicist Gerald O'Neill of Princeton University and Freeman Dyson of the Institute for Advanced Study independently and for different reasons made similar suggestions and calculations about space colonization, all in general agreement with Stroud's practically unknown earlier work. O'Neill presented his ideas directly to NASA for serious consideration. By that time Stroud was no longer much engaged with his earlier notion; he conceded that while intrinsically interesting, space colonies might not escape terrestrial political-military conflicts, and he was confident humanity would survive anyway.

Under Navy auspices Stroud could work on the conscious design of other human habitats and social systems. Human beings living in a submerged nuclear submarine for a long time is one such system. The space colony required even more elaborate design considerations than a submarine, because the designer had to provide for all the physical needs, as well as the psychological and social requirements, of its thousands of inhabitants for long periods of time. In either case the failure to understand human requirements properly, i.e., an inadequate or incorrect psychological or physiological model, could have terrible consequences for the people concerned. Stroud viewed the design and organization of the Polaris missile system as a challenge because it entailed consciously planning a social system involving many people.

Around 1970 Stroud set down his metaphysical assumptions, points that he accepted as, in his own words, "acts of faith" relevant to the pursuit of science. They make some contact with the schools of phenomenology, and even solipsism in philosophy, but they are clearly his own. He began with "that of which I am aware" as the basic given, making no distinction between dreams, so-called external events, etc. (Incidentally, no objective knowledge is possible of any system of which one is not a part.) Stroud assumed that explanation of the phenomena is possible in terms of other phenomena and more or less demonstrable. Lastly, public science requires some communicable, logical form of language. If his focus as a young man had been on life, in 1970 he was more emphatically concerned with intelligent systems. During the intervening years he had been touched by the second World War, marriage and family, the Macy conferences and contact with McCulloch, his work and quarrel at Stanford, the burgeoning development of computer capabilities available in military research, and also the artifacts commonly labeled artificial intelligence devices.

More concerned with pragmatic outcomes than with ontology, Stroud asserted: "A system is what it does." If the actions are calculating, analyzing information, measuring, deductive reasoning, guiding and controlling a ship or missile, then humans and machines—although different in the details of their actions and capabilities—are comparable living systems. It follows from the basic premise that one person can know of another's experience only by inference, and that one knows of a computer's experience on the same basis. Stroud illustrated his attitude by noting that the casette-correspondence between him and me—especially as we had never seen each other—might be that between two clever computers and commented with a chuckle, "not that I would consider that terribly important one way or another." Of course not!

Whether an open system is presumed intelligent, according to Stroud, depends on the nature of the controls the system exercises over the flow of energy and negative entropy through it. Necessary ingredients of any intelligent system: a memory to map the state of a universe distinguishing what is beneficial from what is harmful; sensory capabilities to determine the state of the universe at a particular time; and some control of its own state so as to resist the harmful and favor the beneficial. A plant already contains these minimal features of an intelligent system. An animal, capable of moving about, needs to map a more complex universe and typically requires more elaborate sensory organs and systems of controls. Life itself is the multiply connected domain in space-time, that intelligent system of which all lower-order intelligent systems are subsystems. The notion of negative entropy applies to it.

Stroud will have to write his own book if he wishes to give a full exposition of his thinking and systematic formulations. As I understand his thoughts, they affront and violate the sensibilities of some of us on at least two counts. First, a system defined in terms of what it does, emphasizing energy, negative entropy, and the system's control of these variables, in effect extols artifacts and discounts the special value we put on fellow human beings. Second, an ethic centered on promoting a more scientific civilization appears to favor a neotechnocratic process over democratic political controversy as the means for social change. In fact, thinking primarily of the larger life or humanity creates a relative disregard of the individual's desires, survival, suffering, or happiness. Among the spectrum of Macy participants Stroud is in some respects at the opposite end from the anthropocentric Frank, Mead, Fremont-Smith, and Kubie. Although both Stroud and Bateson see unity in the many living forms of the biosphere, they perceive and respond to that unity in different ways: Bateson favored a minimum of human intervention in natural ecological systems, while Stroud hoped for ever greater intervention by scientifically oriented, technologically sophisticated people or machines.

Filmer Stuart Cuckow Northrop had family roots in Colonial America going back to Joseph Northrop who settled in New Haven, Connecticut, in 1638. Northrop, a leading philosopher in his day, was widely known and was part of the academic establishment. Whereas Bateson was widely recognized only toward the end of his life and posthumously, Northrop's fame was greatest during his own day and has since diminished. In his philosophical thought Northrop sought comprehensiveness, and it became his lifelong preoccupation to reconcile and synthesize the most diverse elements in the realm of ideas. This harmonizing objective led him to explore the nature of science, law, art, politics, capitalism, communism, religion, Asian, Western, and Latin American cultures, and much else. In comparing cultures he spoke of the "free and adventurous" spirit characteristic of the United States, and perhaps felt his own philosophical investigations were animated by that spirit.[26]

He was seventy-five years old, long retired as Sterling Professor of Philosophy and Law at Yale, when I met him at the office he still used at Yale.[27] The only picture on the wall was a photograph of Albert Einstein who, according to Northrop, appreciated the reciprocal relation of epistemology and science.[28] I asked for his recollections of the Macy conferences, but Northrop did not enter into my questions, nor did he look at me directly. Instead he spoke with some intensity, giving an unhalting exposition of the philosophical ideas described in his books. Four hours later I extricated myself, thinking how old men in our society, who have thought and experienced deeply, may need but not find an audience.

Following a stint in the U.S. Army during the First World War, he completed a master's degree at Yale, and worked briefly in Hong Kong with the International Committee of the YMCA. Northrop then went to Harvard to work on his doctorate, under the joint direction of the physical chemist/sociologist Lawrence J. Henderson and the philosopher William Ernest Hocking. He attended Henry Sheffer's seminars on logic. Northrop's dissertation in philosophy, "The Problem of Organization in Biology," dealt with general issues but specifically considered Henderson's description of homeostatic physical mechanisms normally operating to maintain the acid-base balance and the chemical composition of the blood in mammals.

In the 1930s, when he was already on the Yale faculty, Northrop led a seminar for some Yale scientists who had a predilection for philosophy, Henry Margenau and H.S. Burr among them. In Europe at that time quantum theory was widely interpreted as requiring the sacrifice of the principle of causality; Northrop supported and elaborated Henry Margenau's view that quantum theory was perfectly causal, if the classical description of "the state of a particle" were replaced by a different description.[29] Northrop's views are historically interesting, because historians have made a strong case for the suggestion that preferences for causal or acausal interpretation of quantum theory reflected the cultural milieu, which differed greatly in the U.S. and Central Europe.[30]

A duality was at the center of Northrop's view of knowledge: (1) immediate, directly apprehended knowledge, (2) theoretical knowledge derived logically from formal postulates. According to Northrop the two types of knowledge are essentially different; one cannot be derived from the other. But both enter into science, and some kind of correspondence between them is important in ordinary life as in science. Northrop coined the phrase "epistemic correlation" to characterize that correspondence:

> An epistemic correlation is a relation joining an unobserved component of anything designated by a concept by postulation to its directly inspected component denoted by a concept by intuition. This means, also, that an epistemic correlation joins the aesthetic component of a thing to its theoretic component.[31]

From the time of his dissertation Northrop had been concerned with the philosophy of biology. Although much confusion surrounded the topic at the time, Northrop had a clear understanding of how the increase in differentiation accompanying the growth of organisms was compatible with the Second Law of Thermodynamics. The general tendency toward molecular disorder described quantitatively by the Second Law does not preclude the detailed ordering and structuring that occurs spontaneously when, for example, a flowering plant grows from a seed or an embryo develops. Neither chemistry nor thermodynamics, however, provides an adequate explanation or even description of how such a structural ordering process occurs. Some other principle of organization had yet to be discovered, a mathematical theory providing the epistemic correlation to the directly observed and intuitively known phenomenon. A colleague of Northrop's at Yale, the anatomist H.S. Burr, had looked to the electric field within organisms for an explanation of the tendency to organization. In 1935 Burr collaborated with Northrop to write a paper on "the electrodynamic theory of life—the theory that an understanding of biological organization is to be found if we conceive it from the standpoint of the postulates of electromagnetic theory."[32]

Burr's experimental data and the electrodynamic theory were soon transcended by a more convincing explanation based on the notion that genes are macromolecules, carrying information containing instructions to bring about biological organization. In his overestimation of the electrodynamic theory of life Northrop revealed that the critical appraisal of a scientific theory was not his forte. The fundamental point though was Northrop's recognition of the need for a "theory by postulation" to describe biological organization.

He viewed psychiatry and psychology in a similar way. One knows directly one's feelings, fantasies, and thoughts, but an adequate scientific understanding requires a theory derived from a formal set of postulates. If such a theory is to be comprehensive for psychiatry and introspective psychology, the postulates must be expressible in terms of physiological entities and the physical structure of the nervous system.[33] It is then necessary to establish epistemic correlations between the formal theoretical and the directly intuitively known. Since Warren McCulloch was also part of the Yale seminar, Northrop was acquainted with at least one person who was thinking seriously about how to construct a scientific psychiatry and psychology.

Northrop gave philosophical backing to certain efforts in the physical and biological sciences that seemed to him to be epistemologically on the right track. He addressed the human and the social somewhat differently, starting with his knowledge of the methods of the physical and biological sciences and using his distinction between immediate, direct knowledge and theoretical knowledge as a scaffolding. He was concerned with the question of a methodology for the study of societies, and his studies were informed by his search for a unity that could accommodate diverse cultural forms. His approach is evident in a lecture he gave during the Second World War in which he carved out a place for philosophers in connection with world peace.[34]

Northrop began his talk with the assertion that "the presuppositions of a culture determine its empirical manifestations and institutions." He insisted on this questionable premise, according to which ideas or beliefs are the root cause, through all his subsequent writing about societies. Marxists had claimed just the reverse. Lawrence Frank, on the other hand, had in his judicious way allowed for circularity, seeing belief and existing social order as mutually cause and effect. Northrop's approach to world peace, sketched out in 1944 and filled in extensively later in his career, runs along these lines. Philosophical presuppositions differ from one culture to the next. For example, Asian philosophy emphasizes nonaction, intuition, and contemplation, whereas in the West, from Aristotle, Saint Thomas, and Kant to modern science, the emphasis has been on formally constructed, logically reasoned doctrines. Yet an overarching standpoint can be achieved from which differing presuppositions can be reconciled by viewing them as diverse perspectives on the same world rather than as contradictory. If some presuppositions of one culture are incompatible with those of another, then one or the other "has made basic assumptions which are contrary to the facts in that portion of the nature of things to which the theories in question purport to refer." In this way Northrop "provides an empirical criterion for deciding with respect to the goodness or badness of the ethical and institutional norms of two different cultures when these norms are inescapably contradictory.[35] Good norms are in accord with the nature of things. World peace requires identifying good norms of behavior. Northrop concludes: "Philosophy has three tasks with respect to the peace: (1) An analysis of the major cultures of the Western and Eastern worlds which designates the basic theoretical assumptions from which the social institutions and practices that they value proceed. (2) The specification of a common, single set of assumptions possessed of the greater generality which permits the largest possible number of the resultant diverse, traditional assumptions which are logically compatible to be retained and acted upon without conflict. (3) The reconstruction of all the traditional assumptions to the extent that this is necessary, in order to bring them more in accord with the nature of things as this is revealed by contemporary as well as traditional philosophical and scientific knowledge."[36]

Shortly after the end of the war, Northrop described how one could incorporate the Western theoretic component and the Eastern aesthetic component into a religion with "worldwide transforming power."[37] In his magnum opus, *The Meeting of East and West* (1946), Northrop gives a rich and interesting description of the ideologies predominant in the major cultures. To his mind Georgia O'Keeffe's paintings represented a successful synthesis of Western and Asian sensibilities. In any case, it is clear that Northrop had seriously embarked on the three tasks for philosophy he had identified as pertinent to peace.

At this point in his career he attended the first cybernetics meeting, where he proposed that the idea of "the good" be based on science. He did not get very far. Most of the scientists present agreed with Einstein's view that "science cannot create ends and, even less, instill them in human beings; science, at most, can supply the means by which to attain certain ends. . . . We should be on our guard not to overestimate science and scientific methods when it is a question of human problems."[38] A few months after the first meeting Northrop presented a paper on the same topic at a symposium jointly sponsored by the American Association for the Advancement of Science and the American Philosophical Association. The title of his paper indicated his contention, "The Scientific Method for Determining the Normative Social Theory of the Ends of Human Action."[39]

Northrop had come to the cybernetics meetings conscious of a gap in his comprehensive philosophy that reflected a lacuna in Western science itself. Northrop found that the scientific ideas presented at the meetings supplied what had been missing. He incorporated them into his thinking and thereafter gave them a central position in his work. He learned to understand the essentials of the McCulloch-Pitts model, as well as those of the Rosenblueth-Wiener-Bigelow article that had generated the cybernetics conferences in the first place. Northrop had long advocated the primacy of "ideas," though no reasonable theory had yet given them palpable form, much less offered formal postulates from which one could draw logical conclusions that related to neurophysiology, and provided epistemic correlation to our introspectively known thinking and perceiving. The work of Pitts and McCulloch put "ideas" into a tangible, properly scientific—as Northrop understood science—framework. In all his later writing, whether it dealt with politics, anthropology, or philosophy, Northrop found the Pitts-McCulloch model useful, although in his enthusiasm he did not make a critical appraisal of the model's validity. Most important for his philosophy was McCulloch's and Pitts's notion, as he described it, that "the trapped impulses in reverberating circuits are formally equivalent to . . . the introspected facts which we call ideas." In Northrop's conceptual framework the reverberating circuits are the epistemic correlates of introspected ideas, including social norms and scientific theories. Through the motor neurons ideas can "causally determine particular human behavior and, through that human behavior, the character of cultural institutions."[40] Northrop extended the notion of purposive behavior with negative feedback to the situation where the goal to be attained is compliance with a cultural norm or ideology, which are themselves ideas.[41]

While Bateson was using what he learned at the cybernetics conferences to highlight the essential role of paradox and inconsistency in human affairs, Northrop was looking to neurobiology to provide an argument for avoiding inconsistency between one's knowledge of nature and one's ideology:

> Socrates and Plato said in the Republic and at the end of the Timaeus that only the person who identifies the normative philosophy which he uses to define his personal and social purposes with the natural philosophy checked scientifically against the data of the senses from nature can be absolutely good and supremely happy. The reason is clear. With only one philosophy giving instructions to his motor neurons, and that one empirically verified so that he can have confidence in it, such an individual can be a single, a composed, and a whole man.[42]

While Mead emphasized that customs and beliefs gain their merit and meaning only in relation to the particular culture in which they occur, Northrop explored the possibility of a universal norm and criterion for goodness, which would incorporate and transcend all particular cultural norms insofar as they are not based on scientific or logical error, for "we will then, for the first time, be able to talk objectively about the one true and adequate philosophy."[43] With philosophy in effect excluded from the Macy meetings, except for the first, Northrop rarely spoke up. He was scheduled to speak at the ninth cybernetics conference on "trapped universals in reverberating circuits and the escape from the relativity of legal and cultural norms."[44] Northrop got detained in New Haven, and, ironically, Bateson spoke in his place. Bateson would not have agreed with Northrop's thesis. In fact, he spoke on humor and the value of paradox in human dialogue.

The first of the three philosophical tasks laid out by Northrop required extensive research in cultural anthropology. He held up as exemplary Clyde Kluckhohn's study of the Navajo culture, which included an analysis of the philosophical assumptions. Philosophical anthropology was part of the scientific underpinning of Northrop's thinking about U.S. and other foreign policies. Although he had earlier acknowledged the correctness of some aspects of Karl Marx's theories, from the vantage point of his idealistic philosophy Northrop increasingly argued that dialectical materialism is an empirical and logical error, thus contrary to the good, and needs to become extinct. Power-political thinking is another such philosophical error.

Northrop thought it appropriate for philosophers to advise presidents and secretaries of state, save them from ineffective foreign policies, and bring about world peace. The administration of Yale University had long regarded the training of future leaders of the American government as part of its mission. As philosophy professor in the Law School, Northrop was well placed to exert an influence on future American foreign policy. In his later lectures and writings Northrop examined concrete political issues in some detail, especially international affairs, and used his erudition to address them systematically.[45] In these discussion he again invoked the different basic assumptions current in various nations in his interpretation of what had taken place or was likely to occur politically. Although Northrop used Wiener's notions on cybernetics, the two approached political analysis in entirely different ways. Wiener, for example, used cybernetics to argue the advantages of small democratic communities as opposed to large centralized (corporate or government) organizations. Wiener did not seek to advise the political establishment, but rather wanted to inform the general public about the likely impact of new technologies related to cybernetics. He was unwilling to publish his work in Northrop's symposium volume addressed to a political and industrial elite.[46]

Northrop often summered in New Hampshire near Holderness, where Mead and Frank also summered, and they knew each other's views. They were also active participants of the Conferences on Science, Philosophy and Religion held throughout the 1940s. With Franz Boas and Ruth Benedict, Mead had been part of the attempt to overcome the arrogance and absolutism of the colonial legacy in anthropology. As Mead wrote in 1928, "It is unthinkable that a final recognition of the great number of ways in which man, during the course of history and at the present time, is solving problems of life, should not bring with it in turn the downfall of our belief in a single standard."[47] But "relativity" had become a popular catch-word implying a confluence of Einstein's physics and social anthropology and rest-ing on a misunderstanding of both. Mead was at pains to explain that in her view cultural relativity did not imply that "all moral practices are limited in time and place and therefore lack any ultimate validity," but rather that "cultural relativity demands that every item of cultural behavior be seen as relative to the culture of which it is a part, and in the systematic setting every item has a positive or negative meaning and value."[48]

Like Northrop, Mead wished to use the data of anthropology to bring about peaceful relations among nations. Mead treasured pluralism and diversity and abhorred the homogenization of cultures. Whereas Northrop spoke of constructing one world religion that could encompass diverse cultures, Mead's active image of harmony with diversity was "the orchestration of cultures."[49] The fundamental difference in outlook between Northrop and many of the social anthropologists was that between a special variant of Platonism and a view closer to philosophical realism. Mead's mentor, colleague, and friend, Ruth Benedict, when reviewing one of Northrop's books, put it this way:

> Even normative theory expresses men's experiences as they are institutionalized in the family, in the marketplace and in the state. Professor Northrop's volume, however, presents cultures as "determined" by specific philosophical theories, and he inevitably paints an overintellectualized picture. . . . He shows that the United States is what it is because of John Locke. He does not raise the crucial question of why the United States espoused one aspect of Lockean philosophy and England another. If he did, he would have to take seriously "determinants" of culture other than the one he recognizes, and he could hardly say of the present state of the world: "All that is required to end the present demoralization is a philosophy . . . which is adequate to the present stage of knowledge of ourselves and the universe." It is by no means "all that is required." If a world philosophy, conceived in the spirit Professor Northrop indicates in his discussion, ever became basic on the globe, it would be an end product of a state of international spiritual health. The cause of that health would still lie elsewhere. Men then, as always, would espouse ideals in philosophy which had come to be appropriate and urgent in their social experience in the family and in their economic, esthetic, and political life.[50]

Benedict's comment is the more humane and respects down-to-earth realities. Despite his erudition and concern with goodness, Northrop had somehow gone astray.

From the foregoing considerations of Bateson, Stroud, and Northrop, and the mention of others inclined to unify, we can conclude that the notions from the Macy Conferences made excellent building materials for constructing serious scientific-philosophical syntheses. However the building that each one erected, and its framework, depended entirely on that person's purposes, tastes, and craftsmanship.

After the publication of Wiener's *Cybernetics* in 1948 the language and ideas of cybernetics were absorbed throughout the United States. Already in 1949 one of the country's leading poets, Charles Olson, used them in a poem.

. . . how is it,
if we remain the same,
we take pleasure now
in what we did not take pleasure before? love
contrary objects? admire and/or find fault? use
other words, feel other passions, have
nor figure, appearance, disposition, tissue
the same?
        To be in different states without a change
        is not a possibility
We can be precise. The factors are
in the animal and/or the machine the factors are
communication and/or control, both involve
the message. And what is the message? The message is
a discrete or continuous sequence of measurable events distributed in
time. . . .[51]

The language of cybernetics gained popularity as the new communication and computer technologies became everyday objects in people's lives and provided imagery and knowledge, through familiarity, for the analogies. Presumably a commonplace system of metaphors is indicative of the structure of people's experience, and in particular of the focus of their interest and attention. When learning and teaching came to be discussed in terms of transmitting information, or even "bits of information," in analogy to certain processes in digital computers, it shifted attention away from understanding.

Feedback has come to mean information about the outcome of any process or activity. No single word for that general idea seems to have existed in the English language before *feedback* was introduced in the context of cybernetics, and the analogy filled a gap. The ubiquity of feedback meant interaction is everywhere. It shifted attention away from an individualism that had highlighted noncircular cause-and-effect and from the individual person—as if he or she could be independent of others and even independent of chance events occurring in the environment. Still, the word betrays its mechanical origins and encourages ignoring much that happens between people.

Other frequently used metaphors related to cybernetics, such as that we are "programmed," or that our society is one large "system," tend to introduce a bias or, more precisely, a one-sidedness in understanding ourselves and society. In all, the language of cybernetics, like any system of concepts and their associated metaphors, illuminates one facet of our world and experience at the price of masking others.

Although McCulloch and Bateson showed it need not be so, mechanistic metaphors for living, belittling the subjective and the historical, may engender distasteful hypotheses about humans. Such hypothesis may be better suited for manipulation and control than they are for love and understanding.

12

Then and Now

The cybernetics conferences consisted of talk among participants—conversations, presentation of papers, argument and critique—each telling of his or her work to people from other disciplines and seeking to persuade them of its cogency. But is not the "real" work done in the field, in the laboratory, at one's desk—and all that talk peripheral to the development of the human and social sciences? The answer to that question is *No*! Talk is a crucial, centrally important element. The question itself masks a positivistic misconception of the sciences, and especially of the human and social sciences.

Even if Mead, Bateson, McCulloch, and Wiener knew how to transcend this positivistic misconception—a knowledge manifested by their enthusiasm for the conferences—mainstream history and philosophy of science had not yet come to address the function of dialogue, much less that of a small conference of sophisticated participants, leaders in various disciplines. Since the appearance of Thomas Kuhn's *The Structure of Scientific Revolutions* (1963), however, the proper understanding of the nature of natural and social sciences has been the subject of a great deal of controversy and clarification. Examining these controversies, noting the differences of view, and seeking whatever common ground he could find, the philosopher Richard Bernstein identified as essential "the central themes of dialogue, conversation, undistorted communication, communal judgment, and the type of rational wooing that can take place when individuals confront each other as equals and participants."[1] Bernstein's point is that the choice of theory and its interpretation, the evaluation of data, the acceptance or rejection of a paradigm, all these value-laden activities are carried out informally by some sort of elite community—a process that entails achieving a degree of intersubjective agreement through dialogue, and persuasion. Such an emphasis on community—social construction—in describing the nature of scientific development avoids the pitfalls of claiming timeless, objective truth on the one hand, or retreating to individual subjectivity on the other.

The recorded dialogue at the Macy conferences shows that many participants tended to hold to the old, positivist view of the development of science and sought to exclude value issues from the recorded discussion. No matter; these issues popped up anyway and were prominent in arguments outside the conference room. They were in fact the important issues.

Seen in that light, the controversy over Gestalt psychology vs. computer models of perception appears not as an argument about the objective truth but rather as a disagreement about scientific norms, evaluations, and ideologies. The controversies within psychiatry are similarly important ones, and the choice of model for making decisions also has wide implications; these matters, however theoretical, are not confined to a scholarly world but have ramifications in society and are clearly political. One group that promoted a collective point of view was the tribe of Pitts, McCulloch, Wiener, Bigelow, von Neumann, Lorente de Nó, and Rosenblueth. By 1946 they had become converts to a belief in the high value of the set of ideas presented at the first of the ten conferences, and they fostered these ideas through their presentations and comments, as well as through their collective prestige as "hard" scientists. The participants in the conversations instigated and arranged by Larry Frank at his New Hampshire home and elsewhere constituted another elite community. Through conversation and the development of a shared ethos, Mead, Lewin, Frank and others were in the process of "producing" American social science.

The writings of some of he Macy group reveal their shift from traditional positivism, objectivism, and behaviorism toward "social constructionism." Mead, for example, insisted that the artifacts, world views, perceptions, and ideas of a particular culture were relative to and expressive of the culture as a whole. Thus if some conceptions seemed bizarre, it was only because we had not entered that culture. It would require another step to see the practice of anthropology itself as a social construction, a peculiar artifact of our culture comprehensible only relative to it. Similarly, it would have been difficult for Lewin, aspiring to a Galilean psychology, to see that his life-space concept is, after all, a social construct reflecting his interaction with his Berlin colleagues. By 1969, in one of his father-daughter metalogues, Bateson explicitly acknowledged the social element in scientific knowledge construction:

> "*Father:* . . . Scientists are always assuming or hoping that things are simple, and then discovering that they are not.
>
> *Daughter:* Yes, Daddy.
>
> *Daughter:* Daddy, is that an instinct?
>
> *F:* Is what an instinct?
>
> *D:* Assuming that things are simple.
>
> *F:* No. Of course not. Scientists have to be taught to do that.
>
> *D:* But I thought no organism could be taught to be wrong *every* time.
>
> *F:* Young lady, you are being disrespectful and wrong. In the first place, scientists are not wrong every time they assume that things are simple. Quite often they are right or partly right and STILL MORE OFTEN, THEY THINK THEY ARE RIGHT AND TELL EACH OTHER SO. AND THAT IS ENOUGH REINFORCEMENT."[2]

A subtext in the story of the Macy conferences has been the significant role of "clusters of individuals," groups and small communities. Community is an old and recognized political topic, ever since philosophers in ancient Greece called attention to citizenship and the community of the "polis."

In his 1948 discussion of cybernetics, Wiener extolled some types of small communities in diverse cultures, but especially the New England villages he knew first-hand, for their openness and high level of homeostasis: the multitude of feedbacks, patterns of two-way communication, the participatory citizenship and the respect accorded individuals, and everyone's vulnerability to community response. By the same token he saw the centralization of economic and political power—in particular the governments of communist countries and large corporations in capitalist lands—as harmful to the fulfilling of essentially human needs.[3]

One can choose how to think about historical events and what to consider a historical unit: an individual's life and work; an abstract entity such as science or a nation; or, despite an inconvenient complexity, an interacting group of individuals. As Bateson repeatedly pointed out, the natural unit entails complete feedback loops, and these are never coextensive with the boundaries of one individual. Normally the natural and operative unit is a group of strongly interacting individuals. Mead went further. She was convinced that the direction in which events flow at any historical period is set by an interacting group containing at least one person with special talents. She called such a cluster of individuals a unit of cultural microevolution. Mead saw the participants of the cybernetics meetings as just such a group. It was not only Mead and Bateson who thought in terms of groups. Lewin, Fremont-Smith, and Frank did as well.

The Macy meetings, although part of a larger movement, did set new directions for human science research, new directions appropriate to that historical moment in America. Some scientific ideas had been ignored during wartime and needed wide exposure. Other notions from engineering and mathematics had been applied to military purposes or published in reports originally secret. Their inventors were eager to show to the social scientists, and have them confirm, that their design for swords applied equally to building ploughshares. In turn, human scientists had been deflected from academic research to military-related projects during the war years. Returning to more fundamental work, they could use new, suggestive ideas coming from rigorous sciences and engineering. It was not a time of social upheaval but a conservative period of nearly exaggeratedly normal life in the United States, reinforced by official harassment of political dissenters.

Social and political circumstances keep changing. A good candidate for an evolutionary cluster a decade later would be of a different kind, because conditions in 1950 highlighted concerns different from those of 1960. Most participants of the Macy meetings were highly educated middle-aged white males. Participants in the organization and activities of the Student Nonviolent Coordinating Committee, founded in 1960, were college students, youngsters, predominantly black men and women from poor families living in the South. Students from the North joined them. They moved from sit-ins at segregated lunch counters to riding in segregated buses and registering voters in the Deep South. Theirs was a community that had little formal structure and no fixed ideology, but depended for its effectiveness on good communications, personal acquaintance, individual enthusiasm, flexibility, courage, and a youthful willingness to risk all. The very social issues that tended to be hidden in 1950 had come to the fore and exploded by 1960, and an entirely different kind of cluster of individuals and different kind of innovation were required to implement social progress. The ideas from cybernetics, like those of the Civil Rights movement, have generated other events. Both continue, modified and transformed, today and doubtless tomorrow, although present conditions again highlight different needs and concepts.

In closing one must ask what has become of the ideas McCulloch, Pitts, Rosenblueth, Lorente de Nó, Wiener, and von Neumann introduced to the Macy group?

A decade later the cyberneticians of the Macy meetings were no longer a cohesive group. McCulloch and Wiener had quarreled and were no longer on speaking terms. Pitts could be associated with only one or the other; he went with McCulloch, and Wiener would not talk to him either. Pitts subsequently left McCulloch's research group (perhaps feeling that his brains were being picked) and became a recluse. Von Neumann went off on government work dealing with weapons—an activity anathema to Wiener. Lorente de Nó had always been a loner. Only Wiener and Rosenblueth actively continued their friendship.

Although some individuals created a personal synthesis using ideas from cybernetics, no comprehensive unity of science could be derived from it. The subject, the *I*, is omitted in cybernetics, and probably a comprehensive synthesis of human sciences requires it. For the last meeting, McCulloch was asked to present a summary of the points of agreement. He found this an awkward task and prefaced his summary thus:

> Einstein once defined truth as an agreement obtained by taking into account observations, their relations, and the relations of the observers. . . . Unfortunately for us, our data could not be so simply defined. It has been gathered by extremely dissimilar methods, by observers biased by disparate endowment and training, and related to one another only through a babel of laboratory slangs and technical jargons. Our most notable agreement is that we have learned to know one another a bit better, and to fight fair in our shirt sleeves. That sounds democratic, or better, anarchistic, as you have twice reminded me. Aside from the tautologies of theory, and the authority of unique access by personal observation of a fact in question, our consensus has never been unanimous. Even had it been so, I see no reason why God should have agreed with us.[4]

Synthesis did not hold, and as the new ideas and methods entered and were adapted to various disciplines, a kind of centripetal action fragmented them further. The conventional institutional arrangement by departments favors fragmentation and selection from the toolbox of cybernetics of those pieces useful in special disciplines. Since research often goes where funding is available, the development of the field was pulled further out of shape by pursuit of those aspects useful to industrial or military patrons. Military planners have found methods and concepts promoted at the conferences, highly congenial—to the embarrassment of some conferees.[5] Given that some of these concepts were developed during the Second World War and the Cold War, such military interest is not surprising.[6]

The notions with which the cybernetics group grappled pervade many fields today, for instance, ecology. G. E. Hutchinson, who attended the cybernetics meetings, used its fund of ideas, especially circular causality and the self-regulating system with feedback, to describe the biological, chemical, and physical processes in a lake populated with organisms, all coupled to the atmosphere.[7] In the late 1960s Bateson convened a conference (which McCulloch attended) on the effects of humans acting to implement their conscious purposes within ecological systems. Bateson believed that because human entrepreneurs think in terms of linear cause and effect and ignore cybernetic circularities, they will misjudge the consequences of their actions and possibly destroy the environment on which their lives depend. Since then, cybernetic and system-theoretic thinking about ecology has become commonplace. James Lovelock and Lynn Margulis have examined how life—plants, animals, microorganisms—has influenced the chemistry of the atmosphere and the climate, and how life and climate have coevolved. Lovelock's Gaia hypothesis, which relies on a detailed cybernetic analysis, contends that all life on earth acts in concert with the atmosphere to make one self-regulating system that keeps the earth a livable habitat.[8] The validity of the Gaia hypothesis is currently the subject of scientific controversy.

McCulloch's and Pitts's 1943 approach to understanding mind and brain has had enthusiastic successors in the 1980s. They have had the advantage of applying insights from other work that has appeared in the interim—by Hebb, Selfridge, Rosenblatt, Minsky, Papert, Grossberg, A. R. Luria, and Marr, among others. The 1980s enthusiasm was for "parallel distributed processing" neural net models, which are designed to carry out many millions of operations simultaneously and whose computational elements are highly interconnected.[9] In these models memory is not located in one region of the brain but is widely distributed. These models still aspire to link observable cognitive behavior on the one hand to biological neurons and the cerebral cortex on the other. A two-volume survey of the progress in parallel distributed processing appeared in 1986.[10] So far, the link of these models to laboratory neurobiology appears weak. (The Defense Department has a special interest in artificial intelligence, including the 1980s models, and funds some of the work.) In recent years theoretical physicists have been thinking about how to model brain functions (especially using analogies between brain models and the theory of spin glasses), but here too the confrontation with biological data has just begun.[11] But elaboration of the Pitts-McCulloch models of mind, like the original work, are models of only a particular class of functions of mind. They introduce a misleading notion of the nature of mind, because the experiencing subject's sensations and feelings are not taken seriously as part of the human mind.

Consider next the new, transdisciplinary Media Lab instituted at MIT in the 1980s. Onetime Macy participant Jerome Wiesner (who was close to McCulloch, Pitts, and Wiener), Seymour Papert, and Marvin Minsky (important figures in the history of the artificial intelligence approach to mind and brain), are lab associates. The lab received major financial support from such companies as IBM, Apple, Nippon Telephone, and Telegraph, as well as from the Defense Department (DARPA). According to the initial proposal the lab was to provide for "the intellectual mix of two rapidly evolving and very different fields; information technologies and the human sciences." "Graduates will be required to pursue studies in epistemology, experimental psychology, filmmaking, holography, and signal processing, as well as in computer science."[12] A promotional enthusiasm for innovation in high technology and its application to every area of life pervades the lab. It brings sophisticated computer systems to bear on such technical problems as creating a "receptionist" machine that can recognize individuals and respond appropriately, sensibly, and helpfully in all situations. It has a section dealing with electronic publishing responsive to individual readers' interests. It seeks to develop computers that can respond to nonverbal communications such as the direction of a person's glance and the tone of his voice. It deals with improvements in high-definition TV, satellite communication, fiberoptic cable TV, three-dimensional imaging, and data-compression to permit inexpensive transfer of a full-length color film to a compact disk. No wonder the commercial sector is eager to support it; It is a boon for them. An educational tool of interest to some at the Media Lab is a visual representation, with animation, of the dynamic of a natural ecological system, such as a coral reef or a forest. Papert (who worked for a time with Jean Piaget) developed a computer programming language for children (Logo) and tries new educational approaches using computers and other toys in a Boston school. He finds that kids "feel the flexibility of the computer and its power. They can find a rich intellectual activity with which to fall in love."[13] Doubtless IBM and Apple are pleased.

These innovations appear to be driving toward an electronic future in which the individual lives in an environment that neatly simulates human intelligence, human warmth, and human conversation—very different from a social world where genuine relationships with other people are primary. Also missing from this electronic future are close relationships to a natural nonhuman environment—mountains, oceans, trees, animals, small farms. Joseph Weizenbaum and Sherry Turkle have described in their books something of the human peculiarities reinforced or engendered by those whose lives are centered in an artificial intelligence or computer environment.[14] Each of these technologies can radically change the quality and structure of people's lives, especially the balance among electronic environment, human environment, and nonhuman nature. What may be best for IBM is not necessarily conducive to wholesome living. And Langdon Winner has pointed out, that once a technology is in place within a culture, it is a powerful force constraining freedom of choice.[15]

Turkle has identified an implicit, occasionally explicit, premise concerning human psychology among artificial intelligence researchers:

> Ask different AI theorists what are the most important AI theories, and you get different answers. But what is common to all of them is an emphasis on a new way of knowing. The new way of knowing asks that you think about everything, especially all aspects of the mind, in computational terms, in terms of program and information processing. . . . In asserting the primacy of program, artificial intelligence is making a big claim, announcing itself, as psychoanalysis and Marxism had done, as a new way of understanding almost everything. In each case a central concept restructures understanding on a large scale: For the Freudian, the unconscious; for the Marxist, the relationship to the means of production . . . for the AI researcher, the idea of program has transcendent value: it is taken as the key, the until now missing term, for unlocking intellectual mysteries.[16]

The centrality of eros in the Freudian scheme of human motivations is replaced by programmatic problem solving.

Within clinical psychology, however, the "programming" viewpoint is not so important. The ideas of cybernetics and systems theory have shifted the clinical focus from an individual's subjective experience to a larger group, typically a family, viewed as a system with interpersonal communications and feedbacks. The therapist's task is to deal with the pathologies of relationship within that larger system into which he (or she) intrudes. Bateson and his group in Palo Alto had taken the lead in conceptualizing psychotherapies in terms of a family as a whole. Family therapy and, more generally, a systems approach to psychotherapy are now widespread.

Morris Berman, a historian of science, has criticized Bateson's synthesis for its failure to provide an adequate place for political action such as opposition to an oppressive regime.[17] A psychologist who works with a systems approach to psychotherapy is troubled because it tends to ignore individual accountability in human conduct.[18] These limitations of Bateson's synthesis derive in part from his rejection of the commonsense notion that one person may have power over another. The synthesis is incomplete because it shunts aside major features of living: personal accountability and political power. Nevertheless Millard Clements has recently extended Bateson's general ideas in a useful way for application to schooling.[19]

These criticisms do not apply to Wiener's synthesis, also largely based on cybernetics. Wiener said that society does not lack know-how, but "know-what,"—knowing what is worth doing. Many in the Macy group probably agreed. My impression is that society has made no discernable progress in overcoming that lack.

An abundance of technological developments and mass production of devices and systems of communication and computation grew out of the ideas reported by the cyberneticians at the Macy meetings. It became the mainstream of high technology in America and elsewhere in the ensuing decades. Its development has been a challenge to innovative minds, and some of the products continue to be a pleasure and convenience to users, and have become an important part of the infrastructure of our society. Its direction was governed by the market, commercial and military, within the framework of corporate capitalism and, in the United States, a large budget for new military technology. The technology tended only in some instances to address fundamental human needs. It responded more effectively to second-order elaboration of human wishes and dreams.

The growth of cybernetic technologies may be contrasted to the deliberate creation of technologies appropriate to the basic needs of people of all levels of affluence and sensitive to the global and the local ecologies. Beginning about two decades after the first Macy meeting a number of groups of individuals found their ingenuity challenged to develop so-called appropriate technologies, notwithstanding that their products and methods held relatively little interest for the commercial, and none for the military, sector of society. The appropriate technologies, characterized by low cost, local resources, and small scale, typically deal with such basics as energy production and food production. They tend to foster individual autonomy and communities allowing for active participitory citizenship. To mention appropriate technology in the present context, and compare it to other modern technologies, is to sharpen the question of which technologies are worth developing.

Wiener's prediction of a second industrial revolution centered on communication, control, computation, information, and organization has been born out. Robots and computers are transforming the nature of work in industrially advanced countries. The complex character of that change includes deskilling and downgrading the work of some groups of people (such as machinists) and technological unemployment among others (such as middle managers).[20] At the same time the new technologies require training of people in new skills and permit greater flexibility in organizing enterprises. More people can do their work at home. Whether the potential for more humane patterns of work or its opposite is realized in particular industries depends on political forces and decisions.

A group of thinkers and researchers who see themselves as the offspring of the Macy group and who have more interest in concepts than in hardware have been called the creators of "the second cybernetics" by Heinz von Förster. This cybernetics purports to differ from the original in that the observer and the observed are treated as part of the same system; consequently the nonlinearities and paradoxes of self-reference need to be taken seriously. This group's particular concern is the theory of self-organizing systems. Von Förster, who enjoyed working with nonlinearities and paradoxes, has promoted the second cybernetics. A network of people has formed in this new cybernetics tradition; their names appear repeatedly at conferences and in volumes containing articles on self-organization.[21] The group from France includes Jean-Pierre Dupuy, who is concerned with the role of self-organization of social entities in political and economic thought; Isabelle Stengers, who links Ilya Prigogine's dynamical theory of physical systems far from equilibrium with cybernetic thought about self-organizing systems; and the medically trained Henri Atlan, who uses information-theoretical reasoning to characterize biological evolution. The group from England includes Stafford Beer, who has applied cybernetic reasoning to industrial processes and socioeconomic systems and tested them when he assisted Salvador Allende's government in Chile in managing the social and economic organization of the country; Gordon Pask, trained in psychology, who deals with "conscious systems," such as people or certain kinds of computers, but—for example—replaces the behaviorists' measurements of stimulus and response with an analysis of the symbolic-operational structure of "a conversation" and of "understanding" between two individuals as an element of a scientific description of a "conscious system." Two important members of the network, Humberto Maturana (who worked with McCulloch's group at MIT) and his student and coworker Francisco Varela, hail from Chile. They have analyzed in conceptual terms the structure of the network of processes by which an organism functions as an autonomous homeostatic system, maintaining its pattern of organization (i.e., the network of processes) in the presence of continuous perturbations and interactions with other organisms. They assert that the essential characteristics of a living entity is just this network of processes, which take precedence over genetic characteristics and reproductive capabilities. Following in the footsteps of Wiener when he named "cybernetics," they resorted to Greek to name the characteristic organization making for life "autopoiesis" (*self-production*). Finally, in a recent doctor's thesis in the United States, Peter Cariani, who is acquainted with the work of von Förster's network, explores adaptation (of organisms, scientists, or machines) in situations where creativity is required. Specifically, he explores in mechanical terms the means for a system to evolve new sensibilities, such as the capability of constructing or discovering hitherto unknown observables.

The conversation continues.

Appendix

Members of the Cybernetics Group

Original Members of the Group (year of birth)

Gregory Bateson (1904), social science

Julian H. Bigelow (1913), engineering

Gerhardt von Bonin (1890), neuranatomy

Lawrence K. Frank (1890), social science

Frank Fremont-Smith (1895), medicine (foundation executive)

Ralph W. Gerard (1900), neurophysiology

Molly Harrower (1906), psychology[1]

George Evelyn Hutchinson (1903), ecology

Heinrich Klüver (1897), psychology

Lawrence S. Kubie (1896), psychiatry

Paul Lazarsfeld (1901), sociology[2]

Kurt Lewin (1890), social psychology[3]

Rafael Lorente de Nó (1902), neurophysiology

Warren McCulloch (1899), neuropsychiatry

Margaret Mead (1901), anthropology

John von Neumann (1903), mathematics

Filmer S. C. Northrop (1893), philosophy

Walter Pitts (1923), mathematics

Arturo Rosenblueth (1900), physiology

Leonard J. Savage (1917), mathematics

Norbert Wiener (1894), mathematics

Later Members (year of birth; first meeting attended)

Alex Bavelas (1913; fifth), social psychology

Henry W. Brosin (1904; second), psychiatry

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[1] Resigned after fifth conference

[2] Dropped after sixth conference

[3] Died shortly before third conference

Heinz von Förster (1911; sixth), electrical engineering
Donald G. Marquis (1908; second), psychology
Theodore C. Schneirla (1902; second), comparative psychology
Hans Lukas Teuber (1916; fourth), psychology

Guests (meetings attended)
Harold A. Abramson (6)
Nathan W. Ackerman (3)
Vahe E. Amassian (10)
W. Ross Ashby (9)
Yehoshua Bar-Hillel (10)
Morris Bender (4)
Herbert G. Birch (8)
John R. Bowman (8, 9, 10)
Frederick Bremer (1)
Yuen Ren Chao (10)
Eilhardt von Domarus (5)
Max Delbrück (5)
Jan Droogleever-Fortuyn (1)
Erik H. Erikson (3)
Leon Festinger (3)
Frederick Fitch (3)
Roman Jacobson (5)
Clyde Kluckhohn (3, 4)
Dorothy Lee (5)
Joseph C. R. Licklider (7)
Howard S. Liddell (6)
Donald B. Lindsley (6)
William K. Livingston (2)
Duncan Luce (9)
David Lloyd (3, 6)
John Lotz (5)
Donald M. MacKay (8)
Turner McLardy (7)
Frederick A. Mettler (6)
Marcel Monnier (9)
Charles Morris (5)
Henry Quastler (9, 10)
Juan Garcia Ramos (4)
Antoine Remond (9)
David McKenzie Rioch (8)
Ivor A. Richards (8)
Claude E. Shannon (7, 8, 10)
John Stroud (6, 7)
Mottram Torre (9)
W. Grey Walter (10)
Heinz Werner (7)
Jerome B. Wiesner (9)
John Z. Young (9)

Notes

Chapter 1

1. Marty Jezer, *The Dark Ages—Life in the United States 1945–1960* (Boston: South End Press, 1982).

2. E. T. Crawford and A. Biderman, eds., *Social Scientists and International Affairs* (New York: Wiley, 1969), pp. 8–9.

3. Clyde Kluckhohn, *Mirror for Man: The Relation of Anthropology to Modern Life* (New York: McGraw-Hill, 1949), p. 173.

4. A. R. Gilgen, *American Psychology Since World War II* (East Meadow, NY: Glenwood Press, 1982), p. 39.

5. Seymour Sarason, *Psychology Misdirected* (Glencoe: Free Press, 1981), pp. 1–2.

6. Godfrey Hodgson, *America in our Time* (New York: Vintage, 1976), pp. 17–18.

7. Quoted in Hodgson, p. 94.

8. John Dollard, *Caste and Class in a Southern Town,* 3rd ed. (Garden City, New York: Doubleday, 1957), p. xii.

9. Ellen Schrecker, *No Ivory Tower: McCarthyism and the Universities* (New York: Oxford University Press, 1986), p. 339. See also Mary S. McAuliffe, *Crisis on the Left: Cold War Politics and American Liberals, 1947–1954* (Amherst: University of Massachusetts Press, 1978); Michael Rogin, *The Intellectuals and McCarthy* (Cambridge: MIT Press, 1967); Kenneth O'Reilly, "Liberal Values, the Cold War and American Intellectuals," in Athan Theoharis, ed., *Beyond the Hiss Case: The FBI, Congress and the Cold War* (Philadelphia: Temple University Press, 1982). A different perspective is given in Daniel Bell, *The End of Ideology: On the Exhaustion of Political Ideas in the Fifties* (New York: Free Press, 1960), and Richard Pells, *The Liberal Mind in a Conservative Age* (New York: Harper & Row, 1985).

10. An overview is given by Paul Lazarsfeld and Wagner Thielens, *The Academic Mind: Social Scientists in a Time of Crisis* (Glencoe: Free Press, 1958).

11. Schrecker, *No Ivory Tower,* pp. 340–341.

12. Gary Giddins, *Celebrating Bird* (New York: William Morrow & Co., 1987), p. 10.

13. André Hodeir, *Jazz: Its Evolution and Essence* (New York: Grove Press, 1979).

14. Elizabeth Frank, *Jackson Pollock* (New York: Abbeville Press, 1983).

15. For fuller details see Gerald Horne, *Black and Red: W. E. B. DuBois and the Afro-American Response to the Cold War 1944–1963* (SUNY Press, 1986).

16. Paul Forman, "Behind Quantum Electronics: National Security as the Basis for Physical Research in the United States, 1940–1960," *Historical Studies in the Physical and Biological Sciences* 18: 149–229, 1987.

17. General Arthur Trudeau, Army Chief of Research and Development, quoted in Forman, p. 151.

18. Alvin Gouldner, *The Coming Crisis of Western Sociology* (New York: Basic Books, 1970), p. 158.

19. Margaret Mead, *Anthropology, A Human Science* (Princeton: Van Nostrand, 1964), pp. 105–106.

20. Silvan S. Schweber, "The Emergence of American Quantum Electrodynamics after World War II," *Osiris* 2: 265–302, 1986; a fuller exposition is given in Schweber's forthcoming book on the history of quantum field theories. See also Forman.

21. The conferences alluded to were held at Shelter Island (1947), Pocano (1948), and Oldstone (1949); the gifted young physicists were Richard Feynman, Julian Schwinger, and Freeman Dyson.

22. H. F. Judson, *The Eighth Day of Creation: The Makers of the Revolution in Biology* (New York: Simon and Schuster, 1979); James Watson, *The Double Helix* (New York: Atheneum, 1968); Donald Fleming, "Emigré Physicists and the Biological Revolution," in D. Fleming and B. Baylin, eds., *Perspectives in American History II* (Cambridge: Harvard University Press, 1968).

23. Gouldner, *The Coming Crisis.*

24. Ibid., p. 196.

25. Ibid., p. 139.

26. Compare J. G. Morawski, "Organizing Knowledge and Behavior at Yale's Institute of Human Relations," *Isis* 77: 219–242, 1986.

27. Dollard, *Caste and Class in a Southern Town,* p. vii, writes (in 1957): "Since 1937 my research interests have taken a swing away from problems of the community and have increasingly centered in the psychological field. I am therefore no longer up on Negro problems."

28. The most influential book on the topic was Adorno, Frenkel-Brunswick, Levinson, and Sanford, *The Authoritarian Personality* (New York: Harper & Row, 1950).

29. Thomas Rennie and Luther Woodward, *Mental Health in Modern Society* (New York: The Commonwealth Fund, 1948), p. 398.

30. Aside from Tolman, the major neobehaviorist figures are Clark Hull and B. F. Skinner. Any serious assessment of Skinner's approach to psychology must take into account Noam Chomsky's critique in chapter 7 of his collection of essays, *For Reasons of State* (New York: Random House, 1973).

31. Laurence Smith, *Behaviorism and Logical Positivism* (Stanford: Stanford University Press, 1986), p. 5; See also H. Kendler, "Behaviorism and Psychology," and S. Toulmin and D. Leary, "The Cult of Empiricism in Psychology, and Beyond," pp. 121–134 and 594–617 in S. Koch and D. Leary, eds., *A Century of Psychology as Science* (New York: McGraw-Hill, 1985).

32. Steve Heims, *John von Neumann and Norbert Wiener: From Mathematics to the Technologies of Life and Death* (Cambridge: MIT Press, 1980).

33. Ibid., p. 204.

Chapter 2

1. The meeting, called Cerebral Inhibition to indicate an emphasis on neurophysiology, was sponsored by the Macy Foundation and held 13–15 May at the Beekman Hotel in New York City. The foundation paid participants' hotel and travel expenses, but no honorarium. The list of expected participants sent to each on 11 May read: Gregory Bateson, Frank Beach, Carl Binger, Felix Deutsch, Flanders Dunbar, Jule Eisenbud, Milton Erickson, F. Fremont-Smith, Carlyla Jacobsen, Howard Liddell, Lawrence Kubie, Jules Masserman, Margaret Mead, Warren McCulloch, Bela Mittelmann, David Rapoport, Arturo Rosenblueth, Donald Sheehan, George Soule, Robert White, John Whitehorn, Harold Wolff. Although not on that list, Lawrence Frank also attended. See Heinz von Förster, ed., *Cybernetics; Circular Causal and Feedback Mechanisms in Biological and Social Systems*, vol. 8, p. xix (New York: Macy Foundation, 1952). This series of five volumes (volumes 6–10) will subsequently be referred to as *Transactions* (volume 6 refers to the sixth meeting, etc.). Original publication date for each volume was the year following the meeting. These volumes have been reproduced by University Microfilm, Ann Arbor, Michigan.

2. Franklin Fearing, *Reflex Action: A Study in the History of Physiological Psychology* (Williams & Wilkins, 1930; MIT Press, 1970); E. R. Hilgard and D. G. Marquis, *Conditioning and Learning* (New York: Appleton-Century, 1940, 1961).

3. Ernest Rossi et al., eds. *The Essential Milton H. Erickson* (New York: Irvington, 1984).

4. Howard Liddell, *Emotional Hazards in Animals and Man* (Springfield: Charles Thomas, 1956); Liddell et al., "The Comparative Physiology of the Conditioned Motor Reflex," *Comparative Physiology Monograph* 11, no. 51, 1938. The written version of Rosenblueth's talk is A. Rosenblueth, N. Wiener, and J. Bigelow, "Behavior, Purpose and Teleology," *Philosophy of Science* 10: 18–24, 1943.

5. M. Mead, "Cybernetics of Cybernetics," in H. von Förster et al., eds. *Purposive Systems* (New York: Spartan Books, 1968), p. 1.

6. Rosenblueth brought to these conversations not only his years of experience in Walter Cannon's physiology laboratory, but a detailed understanding of Cannon's notion of "homeostasis" and an acquaintance with Cannon's suggestions that the concept could also be applied to a description of society. Wiener and Bigelow had been developing a theory of prediction, which during the war was immediately applied to the "purposeful" aiming and firing

of antiaircraft guns at moving targets. Bigelow brought to their collaboration the engineer's ability to conceptualize as well as to construct models in the metal.

7. Rosenblueth, Wiener, and Bigelow, "Behavior, Purpose and Teleology."

8. Rosenblueth to McCulloch, 21 June 1941.

9. Source material about the conference can be found in the Fremont-Smith archives at the Countway Library in Boston and the McCulloch archives at the Philosophical Library in Philadelphia. The former contains the results of a noisy "sound-scriber" recording of the conference, as well as a partial transcript. After the third conference McCulloch prepared a summary of what transpired at each meeting and sent a copy to all participants. Margaret Mead's archives in the Library of Congress contain notes she made of the conference in a shorthand of her own. Mead's notes are useless because they are undecipherable by anyone except Mead herself, now deceased.

10. Norbert Wiener, *Cybernetics* (New York: Wiley, 1948); John von Neumann and Oskar Morgenstern, *Theory of Games and Economic Behavior* (Princeton: Princeton University Press, 1944).

11. Computer at the Institute for Advanced Study, in Princeton, the first to embody fully the logical structure advocated by von Neumann, became operational only in 1952. The most advanced computer operational in the United States in March 1946 was the ENIAC, developed during the war for the Army Ordnance Department but completed only after the war. That enormous machine, with its 18,000 vacuum tubes, used punched cards for data input, and its instructions were laboriously plugged in by human operators making the connections one at a time. See, e.g., A. W. Burks, "Super Electronic Computing Machine," *Electronic Industries,* July 1946. Internal reports described early thinking about the logical design of advanced machines: A. W. Burks, H. H. Goldstine and J. von Neumann, "Preliminary Discussion of the Logical Design of an Electronic Computing Instrument," Institute for Advanced Study, 28 June 1946: and the still earlier, June 1945 "First Draft of a Report on EDVAC," prepared by von Neumann, which also called attention to the similarity between computer logic and the nervous system as described by McCulloch and Pitts. See also Nancy Stern, *From ENIAC to EDVAC; A Case Study in the History of Technology* (Bedford: Digital Press, 1981).

12. Rafael Lorente de Nó, *A Study of Nerve Physiology* (New York: Rockefeller Institute, 1947).

13. Alan Turing, "On Computable Numbers," *Proceedings of the London Mathematical Society,* 42(2), 230–265, 1936; Emil Post, "Finite Combinatory Processes," *Journal of Symbolic Logic* 1: 103–105, 1936. For historical background see Andrew Hodges, *Alan Turing: The Enigma* (New York: Simon and Schuster, 1983).

14. Warren McCulloch and Walter Pitts, "A Logical Calculus of the Ideas Immanent in Nervous Activity," *Bulletin of Mathematical Biophysics,* 5: 115–133, 1943.

15. Rosenblueth, Wiener, and Bigelow, "Behavior, Purpose and Teleology."

16. For a review of the early history see Otto Mayr, *The Origins of Feedback Control* (Cambridge: MIT Press, 1970).

17. Von Neumann and Morgenstern, *Theory of Games and Economic Behavior.*

18. Wiener's paper, "Time, Communication, and the Nervous System," was presented at the New York Academy of Sciences on 21 October 1946, and appears in its *Annals* 50:197–219,1948.

19. Molly Harrower, *Appraising Personality: An Introduction to the Projective Techniques* (New York: Simon and Schuster, 1952), contains some of that material.

20. Heims, *John von Neumann and Norbert Wiener.* Bigelow, whose most significant work had been in collaboration with either Wiener or von Neumann, tended to stay in the background. His contribution as an engineer and in conversation may have been greater than the published work shows.

21. G. Evelyn Hutchinson, *The Kindly Fruits of the Earth* (New Haven: Yale University Press, 1979).

22. Tracy Putnam at 8–9 February 1946, New York Academy of Sciences conference, The Physico-Chemical Mechanism of Nerve Activity.

23. Yehoshua Bar-Hillel, *Language and Information* (Reading: Addison-Wesley, 1964), p. 6.

24. Wiener, *Cybernetics,* p. 38.

25. Ibid., p. 34.

26. Ibid., p. 189.

27. Seventh conference, *Transactions,* pp. 11–12.

28. A useful review is Margaret Rossiter, "Science and Public Policy since World War II," *Osiris* 1(2): 273–294, 1985.

Chapter 3

1. A. Einstein, "Motiv des Forschens," 1918; quoted and translated in Gerald Holton, *Thematic Origins of Scientific Thought* (Cambridge: Harvard University Press, 1973), pp. 376–377.

2. Among the participants at the conferences described here, the most articulate expositor of the dangers of the new ideas was Norbert Wiener. See his *Cybernetics, The Human Use of Human Beings* (Boston: Houghton Mifflin, 1950) and *God and Golem, Inc.* (Cambridge, MIT Press, 1964).

3. Warren McCulloch, *Embodiments of Mind* (Cambridge: MIT Press, 1965), p. 157.

4. See, e.g., ibid., pp. 3–6, 367.

5. William A. Wallace, "The Philosophical Setting of Medieval Science," in David C. Lindberg, ed., *Science in the Middle Ages* (Chicago: University of Chicago Press, 1978), p. 91.

6. E. von Domarus, "Logical Structure of Mind," in Lee Thayer, ed., *Communication: Theory and Research* (Springfield: Thomas, 1967).

7. E. von Domarus, "The Specific Laws of Logic in Schizophrenia," in J. S. Kasanin, ed., *Language and Thought in Schizophrenia* (Berkeley: University of

California Press, 1944); E. von Domarus, "Ueber die Beziehung des normalen zum schizophrenen Denken," *Archiv für Psychiatrie* (Berlin), 74: 641, 1925; Silvano Arieti, "Special Logic of Schizophrenic and Other Types of Autistic Thought," *Psychiatry* 11: 325, 1948.

8. McCulloch, *Embodiments*, p. 2.

9. McCulloch in Lloyd Jeffress, ed., *Cerebral Mechanisms in Behavior: The Hixon Symposium*, (New York: John Wiley, 1951), p. 32.

10. McCulloch, "The Functional Organization of the Cerebral Cortex," *Physiological Review* 24: 390–407, 1944; McCulloch, "Cortico-cortical Connections," in Paul C. Bucy, ed., *The Precentral Motor Cortex* (Urbana: University of Illinois, 1944), pp. 211–242.

11. F. S. C. Northrop, *Philosophical Anthropology and Practical Politics* (New York: Macmillan, 1960), chapter 3; McCulloch in his "The Beginning of Cybernetics" (*American Society for Cybernetics Forum* 6: 5, 1974) described the seminar as "an interdisciplinary group in the neurosciences, which often touched on cybernetic topics." Northrop's list of participants in that seminar include Clark Hull, H. S. Burr, Mark May, John Dollard, Leonard Doob, Henry Margenau, and Frederic Fitch. McCulloch also attended a Wednesday evening seminar group (around 1936) at the Institute of Human Relations as Yale, known as "Hull's seminar," which included Dollard, Doob, Northrop, and McCulloch as well as Donald Marquis, Neal Miller, Robert Sears, and O. Hobart Mowrer. There Hull proposed a theory of language that did not impress McCulloch, who described it in "The Beginnings of Cybernetics" as only "entertaining." For Hull's seminar see Hull and Mowrer, "Hull's Psychological Seminars, 1936–1938" at the Yale University Library, and Smith, *Behaviorism and Logical Positivism.*

12. According to Jerome Lettvin (MIT lecture, 23 February 1977), Leibniz's *Monadology*, viewed as a work in epistemology, identifies monads as "incorporal automata," in other words formal automata.

13. In his retrospective, "The Beginnings of Cybernetics," Warren McCulloch especially emphasizes Kant and Descartes.

14. Frederic B. Fitch to Heims, 27 September 1973. Fitch later gave some criticism of the 1943 McCulloch-Pitts paper, which he reviewed in *Journal of Symbolic Logic* 9: 49, 1944. Fitch was a guest at the third conference. He had put the "psychological laws" propounded by Hull into a formal logical system. C. L. Hull, *Principles of Behavior* (New York: Appleton Century Croft, 1943). Both Fitch and Hull were part of the Northrop Yale seminar.

15. McCulloch, *Embodiments*, p. 143.

16. Quoted in Mary Catherine Bateson, *Our Own Metaphor* (New York: Knopf, 1972), p. 226.

17. McCulloch, *Embodiments*, p. 220.

18. Johan Huizinga, *Homo Ludens* (Roy Publishers, 1950), p. 3.

19. McCulloch, "Why the Mind is in the Head" *Hixon Symposium*, pp. 56–57.

20. McCulloch, *Embodiments*, p. 200.

21. Ninth conference, *Transactions*, p. 11, gives an indication.

22. Henry Brosin to McCulloch, 24 January 1949.

23. McCulloch, "The Beginnings of Cybernetics"; Jerome Lettvin, interview with Heims, 25 May 1971.

24. Wiener to Henry Allan Moe, September 1945.

25. J. von Neumann, *Hixon Symposium,* pp. 22–23; it has been emphasized by Papert, in his introduction to *Embodiments of Mind,* pp. xviii and xix, that the von Neumann statement is crude and can be misleading. However, more refined statement of the Pitts-McCulloch result requires the elaborate language of formal logic.

26. Kenneth Craik, *The Nature of Explanation* (Cambridge: Cambridge University Press, 1943), pp. 76–77.

27. Among them Jerome Lettvin and, somewhat later, Oliver Selfridge.

28. Wiener to Moe, September 1944.

29. It seems that Pitts and Selfridge had gone over the manuscript for Wiener's *Cybernetics,* but some confusion over the copies led Wiener to send the uncorrected copy to the publisher (Selfridge, interview with Heims, 2 December 1971). But see also N. Wiener, *I am a Mathematician* (Cambridge: MIT Press, 1956), p. 332. According to Wiener's correspondence with Pitts, McCulloch, and Rosenblueth, in spring 1947 Wiener was upset at Pitts, believing he had lost a manuscript of Wiener's. The manuscript was found, and Wiener's suspicion that others were covering up for Pitts allayed. Probably this was the manuscript for a journal article rather than the one for *Cybernetics.* All those were "family arguments" among the core group of neomechanists.

30. Information concerning Pitts derives from recollections of people who knew him. A few letters by him are in the archives of others, and he is the subject of correspondence by McCulloch, Wiener, Rosenblueth, and others. On the whole, however, documentation concerning his life is sparse.

31. Sherry Turkle, *The Second Self: Computers and the Human Spirit* (New York: Simon and Schuster, 1984).

32. McCulloch, *Embodiments,* pp. 216–217.

33. Tracy Putnam in "The Physico-Chemical Mechanism of Nerve Activity," *Annals of the New York Academy of Sciences* 67: 378, 1946.

34. Heims, *John von Neumann and Norbert Wiener.*

35. Wiener to McCulloch, 15 February 1946.

36. W. McCulloch, "Norbert Wiener," *Journal of Nervous and Mental Disease,* 140: 16, 1965.

37. Wiener to Pitts, 12 August 1944; 17 October 1944; Wiener to Rosenblueth, 19 October 1944. The Wiener-McCulloch-Rosenblueth-Pitts correspondence throughout the 1940s provides information about their interrelations.

38. Rosenblueth to Moe, 22 March 1943.

39. Walter Cannon to Chauncy Leake, 15 March 1943. The letter was brought to my attention by Louisa Benton, who has written a thesis on Rosenblueth, "Arturo Rosenblueth: Success or Failure?" (Harvard University, 1986).

40. McCulloch to Fremont-Smith, 23 October 1943; Rosenblueth to Francis Gerty, 26 October 1943.

41. McCulloch to Fremont-Smith, 10 November 1943.

42. Robert Lambert to Moe, 9 August 1945; Robert Morison, diary entry, 21 September 1946, at the Rockefeller Foundation Archives.

43. Wiener, Aiken, and von Neumann to S. S. Wilks, Pitts, E. H. Vestine, W. E. Deming, McCulloch, Lorente de Nó, and Leland E. Cunningham, 4 December 1944.

44. Wiener to Rosenblueth, 24 January 1945. The meeting was held 6–7 January 1945, but in Wiener's *Cybernetics* it is dated incorrectly as occurring in the winter of 1943–44.

45. Von Neumann, report on the meeting, 12 January 1945.

46. Von Neumann to Wiener, 21 April 1945.

47. Wiener to Rosenblueth, 11 August 1945.

Chapter 4

1. Marie Boas Hall, *Robert Boyle on Natural Philosophy* (Bloomington: Indiana University Press, 1965), p. 11; see also George H. Turnbull, *Hartlib, Dury, and Comenius* (Liverpool: University of Liverpool Press, 1947); Thomas Birch, ed., *The Works of the Honorable Robert Boyle* (2nd ed., London, 1772), vol. 6.

2. Bateson to Heims, 7 June 1968.

3. W. Coleman, "Bateson and Chromosomes," *Centaurus* 15: 268 (1971).

4. Gregory Bateson, *Steps to an Ecology of Mind* (SanFrancisco: Chandler, 1972), pp. 379–396 and pp. 73–87.

5. G. Bateson to W. and C. B. Bateson, 21 July 1925, quoted by David Lipset, *Gregory Bateson: The Legacy of a Scientist* (Boston: Beacon Press, 1980), p. 115.

6. See Lipset, *Gregory Bateson,* for more about his brothers.

7. Compare Mary Catherine Bateson, *With a Daughter's Eye: A memoir of Margaret Mead and Gregory Bateson* (New York: William Morrow, 1984).

8. Gregory Bateson, *Naven* (Stanford: Stanford University Press, 1958; 1st ed. Cambridge: Cambridge University Press).

9. Ibid., p. 175.

10. Bateson, *Steps,* pp. 61–127.

11. Bateson, *Naven,* chapter 16 (1936 epilogue).

12. Bateson, *Steps,* pp. 159–176. Bateson's talk was a comment elaborating on a preceding presentation by Mead. Aside from Mead, L. K. Frank, D. G. Marquis, K. Lewin, and D. Lee are also referred to. Mead, Frank, Marquis, and Lewin were four of the social scientists invited to the first Macy conference (8–9 March 1946), and Lee was a later guest. It is all very cozy.

13. Bateson, *Naven,* p. 257.

14. Bateson, *Steps,* pp. 69–70.

15. Bateson, *Naven,* pp. 179–180.

16. Bateson, *Steps,* p. 471.

17. *Current Biography* (New York: H. W. Wilson Co. 1958), entry under Lawrence K. Frank.

18. For Frank's life and work, sources include my interview with him, 23 February 1968; the Milton J. E. Senn Oral History Collection, History of Medicine Division, National Library of Medicine, Bethesda, Maryland; Rockefeller Foundation Archives, Pocantico Hills, New York; Milton Senn, "Insights on the Child Development Movement in the U.S.," Monograph of the Society for Research in Child Development, Vol. 40, Nos. 3–4, August 1975; Steven Schlossman, "Philanthropy and the Gospel of Child Development," *History of Education Quarterly,* fall 1981: 275–299; "Perils of Popularization: The Founding of *Parents Magazine,*" Monograph of the Society for Research in Child Development, 1984; Lucy Sprague Mitchell, *Two Lives* (New York: Simon and Schuster, 1953); Emily Cahan, "Science, Practice, and Gender Roles in Early American Child Psychology," in F. Kessel, ed. *The Past as Prologue in Developmental Psychology* (in press).

19. Schlossman, "Philanthropy."

20. Mary and Larry Frank, *How to Help your Child in School* (New York: Viking, 1950); Mary and Larry Frank, *Your Adolescent at Home and in School* (New York: Viking, 1956). L. K. Frank, *On The Importance of Infancy* (New York: Random House, 1966).

21. Frank to Heims, interview, 23 February 1968.

22. Frank to Walter Cannon, 23 September 1929.

23. Cannon to Frank, 26 September 1929.

24. Frank, *On the Importance of Infancy,* final chapter.

25. Mead, interviews, 1968; Mead, autobiographical sketch in B. Schaffner, ed., *Group Processes,* Transactions of the 1957 Conference (New York: Josiah Macy Foundation, 1958).

26. Quoted in Senn, "Insights."

27. For criticism of that outlook see Christopher Lasch, *Haven in a Heartless World* (New York: Basic Books 1977); Barbara Ehrenreich and Deirdre English, *For Her Own Good: 150 Years of the Experts' Advice to Women* (Garden City: Doubleday, 1978).

28. Michel Foucault's work is *The Order of Things: An Archaeology of the Human Sciences* (New York: Random House, 1970); the quotation is from Hubert Dreyfus and Paul Rabinow, *Michel Foucault: Beyond Structuralism and Hermeneutics,* (Chicago: University of Chicago Press, 1983; 1st ed. 1982), p. 31.

29. Mead to Frank, 10 July 1931; Frank to Mead, 29 July 1931.

30. Jane Howard, *Margaret Mead* (New York: Ballantine, 1984), p. 165.

31. Ibid., p. 166

32. Mary Catherine Bateson, *Our Own Metaphor* (New York: Knopf, 1972), p. 8.

33. Quoted in Laurence Frank's obituary, *The New York Times,* 24 September 1986.

34. Ibid.

35. Bateson, *With a Daughter's Eye,* pp. 43, 47.

36. Robert Sears, quoted in Alfred J. Marrow, *The Practical Theorist: The Life and Work of Kurt Lewin* (New York: Basic Books, 1969), p. 131; Mead herself reported that her talks with Edward Sapir were "just magnificent. He'd say half a sentence and I'd say half a sentence and things just wound up in fireworks that were delightful" (Howard, *Mead,* p. 60).

37. See, for example, Mead's autobiographical *Blackberry Winter: My Earlier Years* (New York: Morrow, 1972), Bateson, *With a Daughter's Eye,* and Howard, *Mead.* Howard gives a considerable number of sources for Mead's life and work.

38. I am indebted to H. Millard Clements for suggesting that designation to me for Mead.

39. See, for example, Margaret Mead, *New Lives for Old* (New York: Morrow, 1960).

40. Margaret Mead, *Coming of Age in Samoa: A Psychological Study of Primitive Youth for Western Civilization* (New York: Morrow, 1928); *Growing up in New Guinea: A Comparative Study of Primitive Education* (New York: Morrow, 1930); *Sex and Temperament in Three Primitive Societies* (New York: Morrow 1935).

41. From Mead's presidential address to the American Anthropological Association, 1960, in Margaret Mead, *Anthropology: A Human Science* (New York: Van Nostrand, 1965), p. 5.

42. Helen Swick Perry, *Psychiatrist of America* (Cambridge: Harvard University Press, 1982).

43. For Sapir's attitude toward psychoanalysis I am indebted to Joan Mark, "The Early Impact of Freud on Anthropology and Sociology in the United States," unpublished ms., 1978.

44. For information about these meetings, see *Proceedings: First Colloquium on Personality Investigation,* held under auspices of the American Psychiatric Association, Committee on Relations with the Social Sciences, 1–2 December 1928, New York City; *Proceedings: Second Colloquium on Personality Investigation,* 29–30 November 1929 (Baltimore: Johns Hopkins University Press, 1929, 1930). See also Perry, *Psychiatrist of America.*

45. Wilhelm Reich, *Character Analysis* (original in German, 1933; New York: Farrar, Straus and Giroux, 1945), preface to first English edition.

46. Max Horkheimer, *Critical Theory* (New York: Herder, 1972); Martin Jay, *The Dialectical Imagination* (Boston: Little, Brown, 1973); Russell Jacoby, *Social Amnesia: A Critique of Conformist Psychology from Adler to Laing* (Boston: Beacon Press, 1975).

47. Mead, interview, 14 October 1968; von Förster, interview, 29 October 1969; Teuber, interview, 30 October 1968.

48. Mead, interview; Margaret Mead and Paul Byers, *The Small Conference: An Innovation in Communication* (The Hague: Mouton, 1968).

49. Teuber to Fremont-Smith, 8 Novbember 1954; McCulloch to Fremont-Smith, 18 November 1954; Mead to McCulloch, 22 November 1954; Teuber to McCulloch, 22 November 1954; Mead to McCulloch, 6 December 1954;

identical letters from Mead to Yuen Ren Chao, W. Grey Walter, and Y. Bar-Hillel, 22 December 1954, and Mead to Fremont-Smith, 3 May 1955.

50. Erwin Schrödinger, *What is Life?* (Cambridge: Cambridge University Press, 1944), p. 60.

51. Heinz von Förster, *Das Gedächtnis* (Vienna: Deuticke, 1948).

52. Sixth meeting, *Transactions*, p. 138, comment by Gerard.

53. The interview took place in Pescadero, California, 2 January 1982.

54. Heinz von Förster, *Observing Systems* (Seaside, California: Intersystems, 1985).

55. Sixth meeting, *Transactions*, p. 91.

56. H. Fletcher, *Speech and Hearing* (New York: Van Nostrand, 1929).

57. See G. A. Miller, F. M. Wiener, and S. S. Stevens, *Transmission and Reception of Sound under Combat Conditions* (Washington, D.C.: Office of Scientific Research and Development, 1946), and a number of articles authored or coauthored by Licklider that appeared in the *Journal of the Acoustical Society of America* and in the *American Journal of Psychology* in the years 1946–1948.

58. Paul Edwards, "Technologies of the Mind," Silicon Valley Research Group Working Paper No. 2 (University of California, Santa Cruz, 1985).

59. C. E. Shannon and W. Weaver, *The Mathematical Theory of Communication* (Urbana: University of Illinois Press, 1949).

60. Seventh meeting, *Transactions,* p. 184.

61. Werner to Frank, 28 December 1935; Frank to Werner, 3 January 1936; Frank to Edna White, 3 January 1936. See appendix B of Senn, "Insights."

62. Margaret Mead, *Continuities in Cultural Evolution* (New Haven: Yale University Press, 1964); see also Mead's autobiographical sketch in Schaffner, ed., *Group Processes.*

63. Mead, *Continuities,* pp. 248, 265.

64. Ibid., p. 272.

65. L. K. Frank et al., "Teleological Mechanisms," *Annals of the New York Academy of Sciences* 50: Ar 189–196, 1948.

66. L. K. Frank, *Nature and Human Nature* (New Brunswick: Rutgers University Press, 1951), pp. 4, 7; for a retrospective overview of the years 1945–1969 see William L. O'Neill, ed., *American Society Since 1945* (Chicago: Quadrangle Books, 1969), especially O'Neill's introduction.

67. See Schweber, "The Emergence of Quantum Electrodynamics," and his forthcoming book on the history of quantum field theories.

68. Frank, *Nature and Human Nature,* p. 156.

69. John Watson, *Psychological Care of Infant and Child* (New York: Norton, 1928).

70. Benjamin Spock, *Baby and Child Care* (New York: Pocket Books, 1946).

71. Bateson, *Our Own Metaphor,* p. 119.

72. Daniel Yankelovich, *New Rules; Searching for Self-Fulfillment in a World Turned Upside Down* (New York: Random House, 1981), p. 1.

73. Ibid., p. xii.

74. Betty Friedan, *The Feminine Mystique* (New York: Dell, 1963).

75. Enrenreich and English, *For Her Own Good.*

76. Howard, *Margaret Mead,* p. 243.

77. Bateson, *With a Daughter's Eye,* p. 53.

78. Lipset, *Gregory Bateson,* pp. 174–176; Howard, *Margaret Mead,* p. 232.

79. "Minimum Requirements for a Theory of Schizophrenia," Second Annual Albert D. Lasker Memorial Lecture, delivered at the Institute for Psychosomatic and Psychiatric Research and Training of the Michael Reese Hospital, Chicago, 7 April 1959. Published in Bateson, *Steps,* p. 265.

80. Ibid., p. 269.

81. Stephen Toulmin, *New York Review of Books,* April 3, 1980, p. 38.

82. Robert Coles, *Erik H. Erikson: The Growth of His Work* (Boston: Little Brown, 1970), p. 33.

83. Erik H. Erikson, *Childhood and Society* (New York: Norton, 1950).

84. Mead to Erikson, 26 February 1947.

85. Bateson to McCulloch, 2 April 1947. Hutchinson wrote independently to McCulloch on 9 April 1947.

86. Mrs. David Resnick to Heims, 24 August 1971.

87. Bateson to Heims, interview, 13 August 1968.

Chapter 5

1. Cardano's book, *Liber de Ludo Aleae* (Manual on Games of Chance), and Galileo's *Sopra le Scoperte de i Dadi* (On Discoveries about Dice) were published only posthumously. For more on Cardano see O. Ore, *Cardano, The Gambling Scholar* (Princeton: Princeton University Press, 1953).

2. For more detail see Lipset, *Gregory Bateson,* chapter 3.

3. In his *Foundations of Statistics* (New York: Wiley, 1954) Savage uses concepts in the imprecise English language for pedagogical purposes to paraphrase mathematical statements.

4. Ninth meeting, *Transactions,* pp. 137–138.

5. William Bateson, *Problems of Genetics* with Historical Introduction by G. E. Hutchinson and S. Rachootin (New Haven: Yale University Press, 1979), pp. xv, 86.

6. Bateson, "Minimum Requirements for a Theory of Schizophrenia," *Steps* pp. 245–269, and "The Role of Somatic Change in Evolution," first published in 1963, reprinted in *Steps,* pp. 346–363.

7. Von Neumann to Wiener, 25 and 29 November 1946.

8. Hutchinson to McCulloch, 9 April 1947.

9. McCulloch, letter "To the Members of the Feedback Conference on Teleological Mechanisms," n.d.

10. Von Bonin to McCulloch, 25 March 1947.

11. Bateson to McCulloch, 2 April 1947.

12. Hutchinson to McCulloch, 9 April 1947.

13. Jacques Monod, *Chance and Necessity* (New York: Knopf, 1971), chapter 4.

14. McCulloch, "To The Members of the Feedback Conference."

15. Judson, *The Eighth Day of Creation*, p. 67.

16. Max Delbrück to Heims, 18 September 1973.

17. The connection between the Second Law and information has a prior history in physics dating from the nineteenth-century "Maxwell demon." Examples of how physicists used the Shannon-Wiener ideas of information are L. Brillouin, *Science and Information Theory* (New York: Academic Press, 1962), and E. T. Jaynes, "Information Theory and Statistical Mechanics," *Physical Review* 106: 620–630, 1957; 108: 171, 1957.

18. Bateson said, "We may call the aggregate of these ideas cybernetics, or communication theory, or information theory or systems theory. The ideas were generated in many places: in Vienna by Bertalanffy, in Harvard [*sic*] by Wiener, in Princeton by von Neumann, in Bell Telephone Labs by Shannon, in Cambridge by Craik, and so on. . . . I think that cybernetics is the biggest bite out of the fruit of the Tree of Knowledge that mankind has taken in the last 2000 years. But most of such bites out of the apple have proved to be rather indigestible—usually for cybernetic reasons." ("From Versailles to Cybernetics," in *Steps*, pp. 469–477.)

19. Savage to Heims, interview, 10 November 1968, New Haven; see also Savage to Fremont-Smith, 2 May 1969.

20. Jaynes, "Information Theory and Statistical Mechanics," in R. D. Levine and M. Tribus, eds., *The Maximum Entropy Formalism* (Cambridge: MIT Press, 1979).

21. J. Ruesch and G. Bateson, *Communication: The Social Matrix of Psychiatry* (New York: Norton, 1951, 1968).

22. Bateson to Heims, interview, Oahu, 13 August 1968.

23. Savage to Heims, interview, 10 November 1968.

24. The first recognition of this circularity is usually attributed to Condorcet in the eighteenth century. See Kenneth Arrow, *Social Choice and Individual Values* (New York: Wiley, 1951).

25. McCulloch, "Heterarchy of Values . . . ," in *Embodiments of Mind;* McCulloch to Bateson, 7 December 1946; McCulloch, "A Recapitulation of the Theory, with a Forecast of Several Extensions," in Frank et al., *Teleological Mechanisms*.

26. McCulloch, "A Recapitulation," p. 263.

27. Bateson to McCulloch, 27 November 1946; see also Ruesch and Bateson, *Communication*, pp. 196–197.

28. L. J. Savage, *Foundations of Statistics*, 2nd ed. (New York: Dover, 1972), pp. 19–21.

29. Manuscript dated 16 May 1963, "For WAW's Hair Let Down."

30. L. J. Savage's brother, I. Richard Savage—also a statistician—made an effort in 1972 to obtain his deceased brother's high school records and document his story "as an excellent example of the school system not being able to recognize potentially competent scholars."

31. L. J. Savage and Arthur Smithies, "A Dynamic Problem in Duopoly," *Econometrics* 8: 130, 1940.

32. Julian Bigelow to Savage, 6 September 1946; Savage to Bigelow, 7 September 1946.

33. "Plans for Research," ms. in connection with a Guggenheim application for a year in Paris to begin in 1951.

34. Von Neumann and Morgenstern, *Theory of Games and Economic Behavior.*

35. The "necessary" concept of probability, differing from both that of the objectivist school and from "personal probability," had been developed especially by J. M. Keynes and independently by R. Carnap.

36. An important implication is that even in situations for which one has little prior information, one can still specify a prior probability distribution reflecting whatever knowledge or anticipation one has. Any new information obtained, for example, by means of an experiment, is then used to sharpen the probability distribution. The mathematical basis for this procedure is a simple and convenient theorem, known as Bayes's theorem, which first appeared in a posthumous paper by Thomas Bayes in 1763. Statisticians relying on a frequency (objectivist) definition of probability in most situations cannot specify prior probabilities, so that Bayes's theorem is of little help to them. Forms of statistics that are not based on a frequency definition, including Savage's statistics, are generally referred to as Bayesian statistics.

37. Savage interviews, New Haven, 10 November 1968 and 2 June 1970.

38. Savage, *Foundations,* 2nd ed., p. viii.

39. Savage, "Difficulties in the Theory of Personal Probability," *Philosophy of Science* 34: 305, 1967.

40. G. E. Hutchinson, *The Enchanted Voyage* (New Haven: Yale University Press, 1962), pp. 154–155.

41. Schaffner, ed., *Group Processes,* autobiographical sketches of participants.

42. G. Bateson, "Social Planning and the Concept of Deutero-Learning," in *Science, Philosophy and Religion,* Second Symposium (New York: Harper & Row, 1942); reprinted in *Steps,* pp. 159–176.

43. Bateson, *Naven;* see the preface and epilogue to the 1958 edition.

44. Bateson interview, Oahu, 13 August 1968.

45. Anatole Broyard, Book Review Section of the *New York Times,* 9 December 1984.

46. S. Heims, "Gregory Bateson and the Mathematicians," *Journal of the History of the Behavioral Sciences* 13: 141–159, 1977; S. Heims, *John von Neumann and Norbert Wiener,* chapters 6 and 7.

47. N. Wiener and A. Rosenblueth, "The Role of Models in Science," *Philosophy of Science* 12: 316–322, 1945.

48. Heims interview with Lazarsfeld, 12 March 1970, New York City; Heims interviews with Savage; also Savage papers, Yale University archives.

49. Lazarsfeld to McCulloch, 14 November 1949.

50. Ruesch and Bateson, *Communication,* chapter 7.

51. Ninth meeting, *Transactions,* pp. 2–3.

52. Ibid., pp. 4–5.

53. Bateson to Wiener, 22 September 1955; Bateson, *Steps,* pp. 475–477; Heims, "Gregory Bateson and the Mathematicians."

54. Bateson, *Steps,* p. 268; Heims, "Gregory Bateson and the Mathematicians."

55. N. Wiener, "Some Moral and Technical Consequences of Automation," *Science* 131: 1355–1358, 1960.

56. Memo to Regents of the University of California, August 1978; reprinted in *Mind and Nature,* p. 240.

57. G. Bateson, "The Logical Categories of Learning and Communication and the Acquisition of World Views," prepared in advance for Wenner-Gren symposium number 41, World Views: Their Nature and Their Role in Culture, 2–11 August 1968.

58. See, for example, the method described by R. Bandler and J. Grinder in *Frogs into Princes* (Moab, Utah: Real People Press, 1979); Bateson wrote the foreword to Bandler and Grinder's earlier volume, *The Structure of Magic I* (Palo Alto, Cal.: Science and Behavior Books, 1975).

59. E. E. Schattschneider, *The Semisovereign People* (Hinsdale, Ill.: Dryden Press, 1975), p. 60.

60. Savage, "Difficulties in the Theory."

61. Eighth meeting, *Transactions,* pp. 191–221.

62. Shannon and Weaver, *The Mathematical Theory of Communication;* Wiener, *Cybernetics.*

63. Eighth meeting, *Transactions,* pp. 181–235; D. MacKay, *Information, Mechanism and Meaning* (Cambridge: MIT Press, 1969). For more on "information" see *Proceedings of a Symposium on Information Theory,* W. Jackson, ed. (London 1950, Lithoprinted by American Institute of Radio Engineers, 1953); *Proceedings of a Symposium on Applications of Communication Theory,* W. Jackson, ed. (London: Butterworth 1953); *Information Theory: Proceedings of the Third London Symposium,* C. Cherry, ed. (London: Butterworth, 1956).

64. D. MacKay, "Man as Mechanism," in D. MacKay, ed., *Christianity in a Mechanistic Universe* (Chicago: Intervarsity Press, 1955).

65. Heims, *John von Neumann and Norbert Wiener,* chapter 12.

66. Stanley Reiser, *Medicine and the Reign of Technology* (New York: Cambridge University Press, 1978), p. 230.

67. Mark Engel in *Steps,* pp. vii–viii; see also Bateson's *With a Daughter's Eye* for a description of her father as teacher; he functioned as teacher in long dialogues in 1975 with Governor Jerry Brown of California, and as member

of the Board of Regents of the University of California, to which he was appointed in December 1976.

Chapter 6

1. Michael Foucault, *Madness and Civilization* (New York: Random House, 1965), pp. 271–272.

2. Among them, dealing mostly with the late eighteenth and nineteenth centuries, are David Rothman, *The Discovery of the Asylum* (Boston: Little Brown, 1971); Andrew Scull, ed., *Madhouses, Mad-Doctors, and Madmen* (Philadelphia: University of Pennsylvania Press, 1981); Michael Donnelly, *Managing the Mind* (London: Tavistock, 1983); Gerald Grob, *The State and the Mentally Ill* (Chapel Hill: University of North Carolina Press, 1966), and *Mental Institutions in America* (New York: Free Press, 1973). These books provide further references to the large literature on the history of madness.

3. G. Mora "The Psychiatrist's Approach to the History of Psychiatry," in Mora and Brand, eds., *Psychiatry and Its History* (Springfield, Ill.: Thomas, 1970); H. Ellenberger, "The Evolution of Depth Psychology," in Gladston, ed., *Historic Derivations of Modern Psychiatry* (New York: McGraw-Hill, 1967); David Satin, "Erich Lindemann: The Humanist and the Era of Community Mental Health," *Proceedings of the American Philosophical Society* 126: 327–346, 1982.

4. Kubie said in 1937, when asked to define his position on Freudian psychoanalysis, that he believed that "psychoanalytic technique, properly employed, uncovers significant dynamic psychological facts not obtainable in any other way at present" and can in certain cases "secure therapeutic results not obtainable in any other way at present" and that "the basic principles of psychoanalytic theory are sound," however much he may disagree with some details of the theory. (Kubie to Abraham Myerson, 20 December 1937.)

5. Edward Jarvis, *Causes of Insanity* (Boston 1851), quoted in Rothman, *The Discovery of the Asylum,*, p. 112.

6. Rothman, *The Discovery of the Asylum,* pp. 110–116.

7. Barbara Sicherman, "The Paradox of Prudence,"in Scull, ed., *Madhouses,* p. 220; the original source is George Beard, *American Nervousness: Its Causes and Consequences* (New York: Putnam, 1881).

8. Emile Durkheim, *Suicide* (New York: Free Press, 1951); the first French edition appeared in 1897.

9. G. Spurzheim, *The Physiognomical System of Drs. Gall and Spurzheim* (London: Baldwin, Cradock and Joy, 1815) and *Observations on the Deranged Manifestations of the Mind or Insanity* (London: Baldwin, Cradock and Joy, 1817); J. F. Gall, *On the Functions of the Brain and of Each of Its Parts* (Boston: Phrenological Library, 1835); George Combe, *Elements of Phrenology* (Boston: Phrenology Library, 1835).

10. Gall, *On the Functions of the Brain,* vol. 2, p. 224, quoted in Roger Cooter, "Phrenology and the British Alienists," in Scull, ed., *Madhouses,* p. 70.

11. Robert Young, *Mind, Brain and Adaptation in the 19th Century: Cerebral Localization and its Biological Context from Gall to Ferrier* (Oxford: Oxford University Press, 1970).

12. Cooter, "Phrenology and the British Alienists," pp. 58–104; Michael Donnelly, *Managing the Mind: A Study of Medical Psychology in Early Nineteenth-Century Britain* (London: Tavistock, 1983); Norman Dain, *Concepts of Insanity in the United States, 1789–1865* (New Brunswick: Rutgers University Press, 1964); Eric Carlson, "The Influence of Phrenology in Early American Psychiatric Thought," *American Journal of Psychiatry* 115: 535–538 (1958); Madeline Stern, *Heads and Headlines; The Phrenological Fowlers* (Norman: University of Oklahoma Press, 1971); John Davies, *Phrenology Fad and Science: A Nineteenth Century American Crusade* (New Haven: Yale University Press, 1955).

13. Young, *Mind, Brain and Adaptation.*

14. Donnelly, *Managing the Mind,* pp. 148–151; Andrew Combe, *Observations on Mental Derangement: being an Application of the Principles of Phrenology to the Elucidation of the Causes, Symptoms, Nature, and Treatment of Insanity* (Edinburgh, 1831).

15. Among them Amariah Brighm, Isaac Ray, Samuel Woodward, and Pliny Earle. See Scull, ed., *Madhouses,* p. 8.

16. Stern, *Heads and Headlines,* p. xiii.

17. L. Kubie, "The Repetitive Core of Neurosis," *Psychoanalytic Quarterly* 10: 23–43, 1941.

18. Kubie to Barbara Ross, 23 October 1970.

19. Kubie to Carl Rogers, 11 October 1947.

20. Kubie to Joanna Knobber, 28 December 1971, quoted in Susan Quinn, *A Mind of Her Own: Life of Karen Horney* (New York: Summit Books, 1987).

21. Quinn, *A Mind of Her Own;* Kubie to Jack Rubins, 11 July and 11 September 1972; for a portrayal of the New York Psychoanalytic Institute, see Janet Malcolm's article in *The New Yorker,* 24 November 1980 (pp. 55–133), and 1 December 1980 (pp. 54–155).

22. Kubie, "Transference Forces," in E. Pumpian-Mirdlin, ed., *Psychoanalysis as Science* (New York: Basic Books, 1952), p. 59.

23. L. Kubie, "A Theoretical Application to Some Neurological Problems of the Properties of Excitation Waves Which Move in Closed Circuits," *Brain* 53: pp. 166–178, 1930.

24. Kubie, "The Repetitive Core of Neurosis."

25. L. Kubie, "Some Unsolved Problems of a Scientific Career," *American Scientist* 41: 596–613, 1953, and 167–204, 1954.

26. L. Kubie, *Neurotic Distortions of the Creative Process* (Lawrence: University of Kansas Press, 1958), p. 85.

27. Kubie to Heims, interview, 24 March 1969.

28. L. Kubie, "The Drive To Become Both Sexes," *Psychoanalytic Quarterly* 43: 349–426, 1974, reprinted in *Symbol and Neurosis,* pp. 191–263.

29. L. Kubie, *Symbol and Neurosis* (New York: International Universities Press, 1978), is a selection of his papers with an introductory overview of his work in psychoanalysis by Eugene Brody.

30. Bateson to Heims, interview, 13 August 1968.

31. Bateson, "Effects of Conscious Purpose on Human Adaptation," in *Steps*, pp. 440–447.

32. Kubie, *Neurotic Distortions*, pp. 2–3.

33. Eugene Taylor, ed., *William James on Exceptional Mental States: The 1896 Lowell Lectures* (Amherst: University of Massachusetts Press, 1984), pp. 163–164.

34. See, for example, Kubie in Pumpian-Mirdlin, ed., *Psychoanalysis as Science*, pp. 48–52.

35. Wiener had said at the first conference that information rather than energy or libido is the appropriate variable in psychoanalysis. See Heims, "Gregory Bateson and the Mathematicians."

36. Ruesch and Bateson, *Communication;* for a detailed and systematic exposition of psychoanalysis in terms of information, feedback, control, and organization see Emanuel Peterfreund, *Information, Systems and Psychoanalysis* (New York: International University Press, 1971).

37. L. Kubie, "The Fallacious Use of Quantitative Concepts in Dynamic Psychology," *Psychoanalytic Quarterly* 16: 507–518, 1947.

38. Heims, *John von Neumann and Norbert Wiener.*

39. Wiener's statement about physical measurements is in fact incomplete: in some physical measurements one utilizes the resonances. To measure the energy levels of an atom or transition rates from one level to another, a good method is to radiate it with light close to the resonance frequencies of the atom. In acoustics the resonances are known as "sympathetic vibrations."

40. Ninth meeting, *Transactions*, p. 49.

41. Frank, Ninth meeting, *Transactions*, p. 72.

42. Warren McCulloch, "The Past of a Delusion," Chicago Literary Club, 1953; reprinted in *Embodiments.*

43. The scientific status of psychoanalysis continues to be the subject of endless discussion. For Kubie's own essay see Pumpian-Mindlin, ed., *Psychoanalysis as Science.* For a more recent defense of psychoanalysis as scientific, see Adolf Grunbaum, *The Foundations of Psychoanalysis: A Philosophical Critique* (Berkeley: University of California Press, 1984).

44. McCulloch, *Embodiments*, pp. 282–283.

45. Michel Foucault, *The Order of Things* (New York: Random House, 1970), pp. 373–386.

46. Ibid., p. 376.

47. McCulloch, *Embodiments*, p. 279.

48. Ibid., p. 373.

49. Michel Foucault, *The History of Sexuality* (New York: Random House, 1978).

50. See, e.g., McCulloch to Papert, 12 February 1964.

51. McCulloch, *Embodiments,* p. 281.

52. Von Domarus, "Logical Structure of Mind," and "The Specific Laws of Logic in Schizophrenia."

53. McCulloch to von Domarus, 8 March 1948.

54. Thayer, ed., *Communication,* p. 350.

55. McCulloch, *Embodiments,* p. 373.

56. An updated report on the work of McCulloch and his group lists the following total grant: Josiah Macy Jr. Foundation, $116,500; Rockefeller Foundation, $115,000; Chemical Warfare, $75,000; and an anticipated Navy grant for $12,000.

57. McCulloch, *Embodiments,* pp. 373–386; McCulloch to Fremont-Smith, 1 June 1948.

58. McCulloch to Fremont-Smith, 1 June 1948; at the sixth meeting, (pp. 196–198 of *Transactions*), McCulloch comments on his experiments to treat neuroses with carbon dioxide.

59. McCulloch, *Embodiments,* pp. 379–380.

60. Ibid., p. 380; H. D. Fabing, *Archives for Neurological Psychiatry* 57: 14, 1947.

61. For an overview and short survey of psychiatric research in the city of Chicago at the time, see, for example, John C. Whitehorn, "A Century of Psychiatric Research in America," in J. K. Hall et al, eds., *One Hundred Years of American Psychiatry* (New York: Columbia University Press, 1944), pp. 167–193.

62. McCulloch, *Embodiments* p. 382; McCulloch to W. Penfield, 11 May 1953.

63. Such a description was first given in 1946 by W. K. Livingston. See his "The Vicious Circle in Causalgia," *Annals of the New York Academy of Sciences* 50: 247–258, 1948.

64. Lashley to McCulloch, 20 January 1954.

65. Klüver to McCulloch, 21 November 1953.

66. McLardy to McCulloch, 21 February 1954.

67. Franklin McLean to Kubie, 15 February 1952; the date of the McCulloch reading was 13 February 1952.

68. Kubie to McLean, 20 February 1952.

69. McCulloch to Kubie, 6 March 1952.

70. Kubie to McLean, 19 March 1954.

71. Fulton to Otto Schmidt, 25 March 1953.

72. Kubie to McLean, 19 March 1954.

73. Kubie to Fulton, 21 March 1953.

74. Ibid.

75. Kubie's tribute to McCulloch on his sixtieth birthday.

76. Molly Harrower to Heims, interview, 22 August 1971, Greenport, New York.

77. Harrower mentioned during the August 1971 interview that she has completed a manuscript titled "The Therapy of Poetry." Some of McCulloch's poems are in *Embodiments,* pp. 319–358.

78. M. Harrower, ed., "Training in Clinical Psychology" (L. Kubie, Chairman), *Transactions,* published by the Macy Foundation (1947) and later reissued by the *Journal of Clinical Psychology.*

79. Harrower to Heims, interview, 22 August 1971.

80. Kubie to Heims, interview, 24 March 1969.

81. Kubie, *Neurotic Distortions,* pp. 68, 73.

82. Ibid., p. 1.

83. As Howard E. Gruber has emphasized, biographical studies of outstanding scientists, writers, and artists lead one to question the emphasis on "the creative process" as universal; it is more appropriate to speak of a variety of creative processes. Gruber, *Darwin on Man,* (Chicago: University of Chicago Press, 1981, p. xx) The diversity is exemplified by the comparison of the two protagonists in Heims, *John von Neumann and Norbert Wiener.*

84. See correspondence between Lillian Smith and Kubie (Kubie archives, Library of Congress); also private communications to the author.

85. Kubie, *Symbol and Neurosis,* p. 200.

86. Kubie, *Vassar Alumni Magazine,* February 1950.

87. Buckley, "Tennessee Williams Survives," in *Atlantic Monthly,* November 1970, p. 102; Donald Spoto, *The Kindness of Strangers* (New York: Ballantine, 1985).

88. "What Price Insight" (1949), a review of *Death of a Salesman,* Kubie archives.

89. Review of *Sanctuary in Saturday Review of Literature* 11: 218, 224–226, 1934.

90. Hemingway to Kubie, 1 May 1935; Kubie to Frederick Danziger, 10 June 1963. The galleys are in the Kubie archives.

91. My emphasis. See also Hemingway galleys.

92. Otto Rank, *Art and Artist* (New York: Knopf, 1932). Esther Menaker, *Otto Rank: A Rediscovered Legacy* (New York: Columbia University Press, 1982).

93. Kubie, *Neurotic Distortions,* p. 85.

94. Kubie to Heims, interview, 24 March 1969.

95. Kubie, "The Drive To Be Both Sexes."

96. Ibid., p. 249.

97. Kubie to Heims, interview, 24 March 1969.

98. Margaret Mead, *Male and Female* (New York: Morrow, 1949).

99. I am indebted to Jonathan Bayliss of Gloucester, Massachusetts, for this permutation.

100. Louise DeSalvo, *Virginia Woolf: The Impact of Childhood Sexual Abuse on Her Life and Work* (Boston: Beacon Press, 1989).

101. For examples of how a person's special gifts, joy, and meaning in life can be destroyed in a "therapeutic" process, see Nigel Dennis's review of *Nadia* by Lorna Selfe, *New York Review of Books,* 4 May 1978, and Oliver Sachs, "The Twins," *New York Review of Books,* 28 February 1985.

102. Foucault, *Madness and Civilization,* pp. 279–289.

103. Ibid., p. 287.

104. Harrower to Kubie, 21 February 1955.

105. For a careful popular exposition see Robert Ornstein and Richard Thompson, *The Amazing Brain* (Boston: Houghton Mifflin, 1986).

106. Jacoby, *Social Amnesia.*

107. A sociological critique of these patterns in American culture is Philip Slater's *The Pursuit of Loneliness: American Culture at the Breaking Point* (Boston: Beacon Press, 1970).

108. Kubie, "The Fallacious Use of Quantitative Concepts in Dynamic Psychology," reprinted in *Symbol and Neurosis.*

109. Jay Haley, "The Art of Psychoanalysis" (1958) and "The Art of Being a Failure as a Therapist" (1969), reprinted in *The Power Tactics of Jesus Christ* (New York: Grossman, 1969), pp. 9–26 and 145–176.

110. Lipset, *Gregory Bateson,* p. 176; Bateson, *With a Daughter's Eye,* p. 49; Howard, *Margaret Mead,* pp. 251–252.

111. G. Bateson, "Epilogue" in Peter Ostwald, ed., *Communication and Social Interaction* (New York: Grune & Stratton, 1977). Coon, a leading anthropologist in his day, put forward the scientifically implausible theory that five human races evolved independently from *homo erectus* to *homo sapiens,* with black people making the transition last. Stephen Jay Gould has mentioned this theory as a prototype example of a nonscientific racist theory. Gould, *Ever Since Darwin* (New York: Norton 1977), p. 238.

112. Lipset, *Gregory Bateson,* pp. 176, 185.

113. Ruesch and Bateson, *Communication,* pp. 12–13.

114. Ibid., p. 170.

115. Ibid., pp. 182–183.

116. Bateson to Wiener, 22 September 1952.

117. Ruesch and Bateson, *Communication,* pp. 263–264.

118. Ruesch and Bateson, *Communication,* pp. 263–264.

119. Wiener, *Cybernetics,* 2nd ed., p. 149.

120. Wiener, *Cybernetics,* 1st ed., p. 173.

121. Wiener, *Cybernetics,* 2nd ed., p. 148.

122. Ruesch and Bateson, *Communication,* p. 266.

123. Ibid., p. 272.

124. Bateson to Wiener, 22 September 1952.

125. Jay Haley to Heims, interview, Philadelphia, 2 April 1970.

126. John N. Rosen, *Direct Analysis* (New York: Grune & Stratton, 1953).

127. Ibid.

128. McCulloch to Bateson, 28 December 1967.

129. Bateson to McCulloch, 5 January 1968.

130. Sixth meeting, *Transactions,* p. 89.

131. Sixth meeting, *Transactions,* pp. 150–161.

132. Bateson, *Our Own Metaphor,* p. 24.

133. Ibid., p. 282.

134. *Perceval's Narrative,* Gregory Bateson, ed. (Stanford: Stanford University Press, 1961), pp. xix–xx and xiv.

135. Ibid., p. xiv.

136. Harrower to Heims, interview, Greenport, New York, 22 August 1971.

137. I thank Perdita Connolly for speaking with the landlady about Pitts early in October 1972, three years after his death.

138. A book of essays honoring Pitts and McCulloch, to which among others Heinz von Förster, Jack Cowan, and Jerome Lettvin have contributed essays, has been circulated in manuscript form.

139. Jay Haley, "Development of a Theory: A History of a Research Project," in C. E. Sluzki and D. C. Ransom, eds., *Double Bind* (New York: Grune & Stratton, 1976); Heims interview with Haley, 2 April 1970; Lipset, *Gregory Bateson,* chapter 12.

140. Haley to Heims, interview, 2 April 1970. The first published and seminal paper on the double-bind theory is Bateson, Jackson, Haley, and Weakland, "Toward a Theory of Schizophrenia," *Behavioral Science* 1: 1956, reprinted in *Steps,* pp. 201–227.

141. Bateson to Wiener, 22 September 1952.

142. Bateson to Heims, 17 August 1977; a fuller description of the Bateson-Wiener dialogue is given in Heims, "Gregory Bateson and the Mathematicians."

143. G. Bateson, "Critical Evaluations," *International Journal of Psychiatry* 2: 415–428, 1966.

144. Lipset, *Gregory Bateson,* chapter 12; Haley, "Development of a Theory"; Don Jackson, "The Question of Family Homeostasis," *The Psychiatric Quarterly* Supplement, 31: 79–90, 1957; Jay Haley, "The Family of the Schizophrenic: A Model System," *Journal of Nervous and Mental Disease* 129: 357–374, 1959.

145. Sharon Lamb, "Acts without agents. Harm without guilt." Submitted manuscript, 1990.

146. G. Bateson, "The Cybernetics of 'Self': A Theory of Alcoholism," *Psychiatry* 34: 1–18, 1971; reprinted in *Steps,* pp. 309–337.

147. Ibid., *Steps,* p. 320.

148. G. Bateson, Introduction to Richard Bandler and John Grinder, *The Structure of Magic I* (Science and Behavior Books, 1975).

149. Sluzki and Ransom, eds., *Double Bind,* p. 106. Bateson discusses the concepts "pleroma" and "creatura" in terms of cybernetics in his 1970 Korzybski lecture (*Steps,* pp. 448–465). Put crudely, "pleroma is the world in which events are caused by forces and impacts," the world of substance. The creatura is akin to the world of "mind."

150. Sluzki and Ransom, eds., *Double Bind,* p. 78n.

151. Lipset, *Gregory Bateson,* p. 226.

152. Some types of family therapy that later evolved from ideas of Bateson and coworkers are not free of manipulation.

153. H. Klüver, addendum to "Psychology at the Beginning of World War II," *The Journal of Psychology* 28: 406–408, 1949; *Mescal and Mechanism of Hallucination* (Chicago: University of Chicago Press, 1966), p. ix.

154. L. K. Frank, "Some Postwar Social Trends Which Are of Special Interest to Orthopsychiatry," *American Journal of Orthopsychiatry* 16: 5–7, 1947.

155. Haley, "The Art of Psychoanalysis"; see also, "The Art of Being a Failure as a Therapist."

156. Ehrenreich and English, *For Her Own Good.*

157. Thomas Szasz, *The Myth of Mental Illness: Foundations of a Theory of Personal Conduct* (New York: Harper & Row, 1974), and *The Myth of Psychotherapy* (Garden City: Doubleday, 1979).

158. Recently, however, investigators have systematically compared the relative efficacy of different types of treatment for particular disorders. See, for example, *The New York Times,* 12 December 1989, p. C3.

Chapter 7

1. Lewis Coser, *Men of Ideas: A Sociologist's View* (New York: Free Press, 1965), p. 343.

2. *Josiah Macy Jr. Foundation Six Year Review, 1930–1936* (New York, 1937), pp. 13–14.

3. Ibid., p. 21.

4. Ibid., p. 29.

5. *Twentieth Anniversary Review of Josiah Macy Jr. Foundation, 1950.* (New York, 1950).

6. Benjamin V. White, with Richard J. Wolfe and Eugene Taylor, *Stanley Cobb: A Builder of the Modern Neurosciences* (Boston: Francis A. Countway Library, 1984), p. 167.

7. Lawrence Kubie, "Tribute to Frank Fremont-Smith, M. D.," typescript, March 1969; see also the inside cover of first issue of *Psychosomatic Medicine,* 1939.

8. Harold Abramson, "The Fremont-Smith Effect: A Historical Note," *Journal of Asthma Research* 8: 77–87, 1970.

9. Ibid., pp. 78–79.

10. John Marks, *The Search for the 'Manchurian Candidate': The CIA and Mind Control* (New York: Times Books, 1979) pp. 59–65, 120–121; other books de-

tailing aspects of drugging unsuspecting individuals include Alan Scheflin and Edward Opton, *The Mind Manipulators* (New York: Paddington Press, 1978), Walter Bowart, *Operation Mind Control* (New York: Dell, 1978), and Harvey Weinstein, *A Father, A Son and the CIA* (Toronto: J. Lorimer, 1988).

11. H. Abramson, ed., *Problems of Consciousness* (New York: Macy Foundation, 1950–1955); H. Abramson, ed., *Neuropharmacology* (New York: Macy Foundation, 1954–1960).

12. Marks, *The Search for the 'Manchurian Candidate.'*

13. Sixth meeting, *Transactions,* p. 151; see also comments by Abraham, pp. 101–105.

14. Numerous documents from physicists who worked on the bomb testify to this picture. See, for example, the oral histories at the Center for History of Physics in New York City, or Lawrence Badash, ed., *Reminiscences of Los Alamos, 1943–1945.*

15. *The Josiah Macy Jr. Foundation 1930–1955, A Review of Activities* (New York: 1955), p. 10.

16. An important exception published a few years ago after the Macy meetings is the sociological study by Lazarsfeld et al., *The Academic Mind.*

17. H. Cantril, ed., *Tensions That Cause War* (Urbana: University of Illinois Press, 1950).

18. H. S. Sullivan, *The Fusion of Psychiatry and Social Science* (New York: Norton, 1964), especially chapters 16, 17, and the introduction by H. S. Perry. In his work linking psychiatry and social science, Sullivan had derived both moral and financial support through L. K. Frank.

19. *Tensions that Cause War.*

20. For more on Horkheimer and ideas about psychiatry similar to this, see Martin Jay, *The Dialectical Imagination* (Boston: Little Brown, 1973); Jacoby, *Social Amnesia.*

21. Minutes of 17 December 1943 meeting of the Joint Committee on Post War Problems and Opportunities, written by George S. Stevenson.

22. Minutes of 9 February 1944 meeting of the Joint Committee on Post War Problems and Opportunities.

23. *International Congress on Mental Health, London 1948,* vol. 1: *History, Development and Organization* (London: H. K. Lewis; New York: Columbia University Press).

24. The initiator from the British side was John Rees (1889–1969), psychiatrist and, from 1932 to 1940, director of the Tavistock Square Clinic. During the war years he had been in charge of psychiatry for the medical services of the British Army. He came from a religious background, "which led him to accept without conflict the image of himself as a missionary" (see Kubie to A. J. Ayer, 23 February 1968, and the enclosed summary of Rees's autobiographical "Reflections").

25. *International Congress on Mental Health, 1948,* vol. 1.

26. Margaret Mead, ed., *Cultural Patterns and Technical Change* (New York: New American Library, 1954).

27. Sixth meeting, *Transactions*, p. 11.

28. McCulloch to Fremont-Smith, 10 January 1949.

29. T. S. Szasz, *Ideology and Insanity: Essays on the Psychiatric Dehumanization of Man* (New York: Doubleday, 1970), pp. 35–39. He speaks of the tendency "to replace a clear political vocabulary with an obscure psychiatric semantic, and a pluralistic system of moral values with a singularistic mental health ethic. . . . The promoter of mental health now emerges as a social engineer on the grand scale: he will be satisfied with nothing less than gaining license to export his own ideology to a world market." See also Szasz's earlier *The Myth of Mental Illness*.

30. See, for example, Fanon's 1956 essay "Racism and Culture," in Fanon, *Toward the African Revolution* (New York: Grove Press, 1967), pp. 31–44.

31. Wiener, *Cybernetics*, p. 190.

32. Edwards, "Technologies of the Mind." See also Donna Haraway, "The High Cost of Information in Post World War II Evolutionary Biology," *Philosophical Forum* 13: 244–278, Winter–Spring 1981–82.

33. Connections between Marxist dialectical materialism and cybernetics were not emphasized at the time. Later on, however, Bateson mentioned Marxist analysis as in some respects a precursor of the more general cybernetics; see Bateson, *Mind and Nature* (New York: Bantam Books, 1979). In the Soviet Union the relation of Marxist thought to cybernetics later became the subject of much discussion.

Chapter 8

1. Wiener, *Cybernetics* and *The Human Use of Human Beings;* Heims, *John von Neumann and Norbert Wiener*, pp. 311–312. Robert Lilienfeld, *The Rise of Systems Theory* (New York: Wiley, 1978), documents the tendency to use cybernetic language to promote centralization of power, but his picture of Wiener's thought is misleading.

2. Loren Graham, *Science and Philosophy in the Soviet Union* (New York: Vintage, 1974); R. David Gillespie, "Politics of Cybernetics in the Soviet Union," in A. Teich, ed., *Scientists and Public Affairs* (Cambridge: MIT Press, 1974).

3. Ninth meeting, *Transcript*, p. 6.

4. Schrecker, *No Ivory Tower*, especially pp. 259–264, 333–337; Sigmund Diamond, "The Arrangement: The FBI and Harvard University in the McCarthy Period," in Theoharis, ed., *Beyond the Hiss Case*.

5. Alex Inkeles, "Clyde Kluckhohn's Contribution to Studies of Russia and the Soviet Union," in W. Taylor, J. Fischer, and E. Vogt, eds., *Culture and Life: Essays in Memory of Clyde Kluckhohn* (Carbondale: Southern Illinois University Press, 1973). Bibliographies of Kluckhohn's work are available in Richard Kluckhohn, ed., *Culture and Behavior: Collected Essays of Clyde Kluckhohn* (New York: Free Press, 1962), and T. Parsons and E. Vogt, "Clyde Kluckhohn" *American Anthologies* 64, 140–161, 1962.

6. Diamond, "The Arrangement," pp. 346–347.

7. Parsons in Taylor, Fischer, and Vogt, eds., *Culture and Life*, p. 36.

8. Jon Wiener, "Talcott Parson's Role: Bringing Nazi Sympathizers to the U.S.", *The Nation,* 6 March 1989, cover page and pp. 306–309. The primary source for the article is the Parsons-Kluckhohn correspondence in 1948 and 1949.

9. In his *Mirror for Man* (1949), Kluckhohn acknowledges "profound obligations" to, among others, Bateson, Mead, and Frank.

10. Barbara Heyl, "The Harvard Pareto Circle," *Journal of the History of the Behavioral Sciences* 4: 316–334, 1968; B. Barber's introduction to *L. J. Henderson on the Social System* (Chicago: University of Chicago Press, 1970); T. Parsons, "On Building Social Systems Theory: A Personal History," *Daedalus* 99: 826–881, 1970.

11. L. Henderson, *Pareto's General Sociology: A Physiologist's Interpretation* (Cambridge: Harvard University Press, 1935); C. Russett, *The Concepts of Equilibrium in American Social Thought* (New Haven: Yale University Press, 1966).

12. Parsons, "Personal History"; private communications from several Harvard faculty members.

13. Parsons, "Personal History."

14. Transcript, "Plenary Sessions of the Conference on Transforming Conceptions of Modern Science," 10–14 September 1969, Bellagio, Italy, American Academy of Arts and Sciences, p. 161.

15. Paul Lazarsfeld, *Main Trends in Sociology* (London: George Allen and Unwin, 1973), pp. 52–54; first published as Chapter 1 of *Main Trends of Research in the Social and Human Sciences* (UNESCO, 1970).

16. Quoted in Kluckhohn, *Mirror for Man.* p. vii.

17. Ibid., p. 259.

18. Ibid., p. 270.

19. Karl Polanyi, *The Great Transformation* (New York: Rinehart, 1944).

20. Lewis Coser, *Refugee Scholars in America* (New Haven: Yale University Press, 1984) p. 172.

21. Kluckhohn, *Mirror for Man,* p. 273; portions of the book were written years before publication.

22. For an introduction to Merton, see Robert K. Merton, *On Theoretical Sociology: Five Essays Old and New* (New York: Free Press, 1967).

23. Ibid., p. 75 (written in 1948).

24. Robert K. Merton, "Sociology of Science: An Episodic Memoir," in Merton and Jerry Gaston, eds., *The Sociology of Science in Europe* (Carbondale: Southern Illinois University Press, 1977).

25. Ibid., pp. 47–49.

26. Coser, *Refugee Scholars,* p. 111.

27. Marie Jahoda, Paul Lazarsfeld, and Hans Zeisel, *Die Arbeitslosen von Marienthal* (Leipzig 1932).

28. Theodor Adorno, "Scientific Experience of a European Scholar in America," and Paul Lazarsfeld, "An Episode in the History of Social Research: A

Memoir," in D. Fleming and B. Baylin, eds., *Perspectives in American History II* (Cambridge: Harvard University Press, 1968), p. 338–370, 270–337.

29. Ralph Linton, ed., *The Science of Man in the World Crisis* (New York: Columbia Univ. Press, 1945), p. vii.

30. Paul Lazarsfeld and Genevieve Knupfer, "Communications Research and International Cooperation," in Linton, ed., *The Science of Man,* pp. 465–495.

31. Marie Jahoda, "PFL: Hedgehog or Fox?," in Merton, Coleman, and Rossi, eds., *Qualitative and Quantitative Social Research* (New York: Free Press, 1979), pp. 3–9.

32. Coser, *Refugee Scholars,* p. 110.

33. A. Quetelot, *Physique Sociale* (1869); P. Lazarsfeld, *Isis* 52; 277–333, 1961.

34. James Coleman, introduction to P. Kendall, ed., *The Varied Sociology of Paul F. Lazarsfeld* (New York: Columbia University Press, 1982), p. 5.

35. Lazarsfeld et al., *The Academic Mind,* pp. 72–73.

36. Ibid., p. vi.

37. Schrecker, *No Ivory Tower,* p. 339.

38. Lazarsfeld, *Main Trends in Sociology.*

39. Ibid., p. 73.

40. C. Wright Mills, *The Sociological Imagination* (Oxford: Oxford University Press, 1959), p. 73.

41. Ibid., p. 101.

42. C. Wright Mills, *White Collar* (Oxford: Oxford University Press, 1951); *The Power Elite* (Oxford: Oxford University Press, 1956); *The Cause of World War Three* (London: Secker & Warburg, 1958); *The Sociological Imagination.*

43. Paul M. Sweezy, "Power Elite or Ruling Class," *Monthly Review* Pamphlet Series No. 13 (New York: Monthly Review Press, 1956).

44. For references see Schrecker, *No Ivory Tower,* p. 380, note 68.

45. Lazarsfeld, "Circular Processes in Public Opinion," abstract for the October 1946 session of the New York Academy of Sciences and probably also the basis for his talk at the 20 September special social science subconference of the Macy group.

46. Summary of the second Macy meeting, prepared by Warren McCulloch (mimeographed).

47. Wiener, *Cybernetics,* p. 24.

48. A full bibliography of Lazarsfeld's work is given in Merton, Coleman, and Rossi, eds., *Qualitative and Quantitative Social Research.* His books, mostly with coworkers, include Lazarsfeld, *Radio and the Printed Page* (New York: Duell, Pearce and Sloan, 1940); Lazarsfeld, Berelson, and Gaudet, *The People's Choice* (New York: Duell, Sloan and Pearce, 1944); Lazarsfeld and Field, *The People Look at Radio* (Chapel Hill: University of North Carolina Press, 1946); Berelson, McPhee, and Lazarsfeld, *Voting* (Chicago: University of Chicago Press, 1954); Katz and Lazarsfeld, *Personal Influence* (Glencoe: Free Press, 1955); *The Academic Mind;* Lazarsfeld and Kendall, *Radio Listening in America* (New York: Prentice-Hall, 1948); Lazarsfeld, Dahl, and Haire, *Social Science*

Research on Business (New York: Columbia University Press, 1959); Lazarsfeld and Sieber, *Organizing Educational Research* (New York: Prentice-Hall, 1964); Lazarsfeld and Henry, *Latent Structure Analysis* (Boston: Houghton Mifflin, 1968).

49. See, e.g., Katz and Lazarsfeld, *Personal Influence.*

50. See Leo Huberman and Paul M. Sweezy, *Introduction to Socialism* (New York: Monthly Review Press, 1968), p. 98, from a talk by Huberman presented 15 December 1950 at a meeting of Monthly Review Associates.

51. Ibid., p. 31.

52. John Kenneth Galbraith, *American Capitalism* (Boston: Houghton Mifflin, 1952); *The Affluent Society* (Boston: Houghton Mifflin, 1958).

53. Pells, *The Liberal Mind,* pp. 385–387.

54. Howard Zinn, *A People's History of the United States* (New York: Harper & Row, 1980), pp. 435–459; see also Barton J. Bernstein, "America in War and Peace: The Test of Liberalism," in Bernstein, ed., *Towards a New Past: Dissenting Essays in American History* (New York: Random House, 1968).

55. Quoted in Zinn, *A People's History,* p. 440.

56. Dollard, *Caste and Class in a Southern Town,* p. xii.

57. Kurt Lewin, *Resolving Social Conflicts: Selected Papers on Group Dynamics* (New York: Harper & Row, 1948), p. 104.

58. Ibid., p. 215.

59. Ibid.

60. Franz Fanon stressed the importance of not reducing racism to psychology in his 1956 essay, "Racism and Culture": "Racism is only one element of a vaster whole: that of the systematized oppression of a people. . . . The habit of considering racism as a mental quirk, as a psychological flaw, must be abandoned."

61. Bernstein, ed., *Towards a New Past,* p. 307.

62. The lawyers on the side favoring desegregation (Thurgood Marshall and Robert Carter) hoped that Gordon Allport or Otto Klineberg would testify, but neither came. For more on social scientists and the Supreme Court decision see Richard Kluger, *Simple Justice: The History of Brown vs. Board of Education and Black America's Struggle for Equality* (New York: Knopf, 1976).

63. David Krech and Richard Crutchfield, *Theory and Problems of Social Psychology* (New York: McGraw-Hill, 1948).

64. *New York Times,* 24 March 1949, p. 4. For Einstein's view of socialism see his "Why Socialism," *Monthly Review,* May 1949.

65. From reading Daniel Bell's *End of Ideology* or Pells's *The Liberal Mind* one might get the false impression that all significant left intellectuals gave up their views after the second World War. The four hundred sponsors include sufficient counterexamples to require that the Bell/Pells picture be altered.

66. Pells, *The Liberal Mind,* pp. 121–124.

67. James Aronson, *The Press and the Cold War* (New York: Bobbs-Merrill, 1970), pp. 52–54; *The Autobiography of W. E. B. DuBois* (International Publications, 1968), pp. 349–350.

68. Aronson, *The Press and the Cold War,* p. 55.

69. DuBois joined the Communist Party later and then left the United States to live in Ghana.

Chapter 9

1. K. Lewin, *Principles of Topological Psychology* (New York: McGraw-Hill, 1936), p. 13.

2. P. A. Schilpp, ed., *The Philosophy of Ernst Cassirer* (New York: Tudor, 1958), p. 283.

3. Michael Sokal, "The Gestalt Psychologists in Behaviorist America," *American Historical Review* 89: 1240–1263, 1984.

4. Ernest Hilgard and Donald Marquis, *Conditioning and Learning* (New York: Appleton-Century, 1940).

5. T. C. Schneirla, "Levels in the Psychological Capacities of Animals," in Roy Sellars, V. J. McGill, and Marvin Farber, eds., *Philosophy for the Future: The Quest of Modern Materialism* (New York: Macmillan, 1949), reprinted in *Selected Writings of T. C. Schneirla* (San Francisco: Freeman, 1972). I thank Deborah Gorden for a helpful conversation concerning Schneirla's work. Donna Haraway gives another perspective on the Macy conferences in relation to insect studies in "The High Cost of Information in Post World War II Evolutionary Biology," *The Philosophical Forum* 13: 244–278, 1981.

6. Molly Harrower, *Kurt Koffka: An Unwitting Self-Portrait* (Gainesville: University of Florida, Gainesville Press, 1983); "Koffka's Rohrschach Experiment," *Journal of Personality Assessment* 35: 103–121, 1971; Heims interview with Harrower, Long Island, 20 August 1971.

7. Lewin to McCulloch, 25 February 1946.

8. Shelley Patnoe, *A Narrative History of Experimental Social Psychology: The Lewin Tradition* (New York: Springer, 1988).

9. J. M. Mandler and G. Mandler, "The Diaspora of Experimental Psychology: The Gestaltists and Others," in D. Fleming and B. Baylin, eds., *The Intellectual Migration: Europe and America, 1930–1960* (Cambridge: Harvard University Press, 1968), p. 400.

10. Ibid., p. 375.

11. Heinz von Förster, "Thoughts and Notes on Cognition," in Paul Garvin, ed., *Cognition—A Multiple View* (New York: Spartan Books, 1976), pp. 25–48.

12. J. von Neumann: Letters to Wiener, 25 and 29 November 1946 (MIT archives); also his paper at the Hixon Symposium; *Cybernetics* describes Wiener's viewpoint. McCulloch's and Pitts's view have been described in chapter 3.

13. Kenneth Craik, *The Nature of Explanation* (Cambridge: Cambridge University Press, 1943) p. 114.

14. McCulloch's summary of the first three meetings.

15. Peter Gay, *Weimar Culture* (New York: Harper & Row, 1968), p. 3. The characterization of Weimar culture in terms of "the outsider as insider" is based on Gay's book.

16. Georg Simmel, *Conflict* (Glencoe: Free Press, 1955); Lewis Coser, *The Function of Social Conflict* (New York: Free Press, 1956) spans Simmel and Lewin on the issue of social conflict. See *Masters of Sociological Thought* (New York: Harcourt Brace Jovanovich, 1971), pp. 177–216.

17. In the discussion of the history of Gestalt psychology I am relying heavily on Mitchell Ash, "The Emergence of Gestalt Psychology," Ph.D. thesis, Harvard, 1982; also Mandler and Mandler, "The Diaspora of Experimental Psychology."

18. Kurt Lewin, "Carl Stumpf," *Psychological Review* 44: 189–194, 1937; Stumpf's "Selbstdarstellung" in Carl Murchison, ed., *History of Psychology in Autobiography,* vol. I, pp. 389–441 (1930); Herbert Spiegelberg, *The Phenomenological Movement,* 3rd ed. (The Hague: Martinus Nijhoff, 1982), chapter 2. It would be a mistake to regard even nineteenth-century German psychology as entirely isolated from American psychology. Stumpf (1848–1936) had met William James in 1882, and the two became friends, extolled each other's work, and had a regular correspondence until James's death.

19. Connections and affinity between Gestalt psychology and Husserl's phenomenology are examined in Aron Gurwitsch, *Studies in Phenomenology and Psychology* (Evanston: Northwestern University Press, 1966).

20. W. Dilthey, "Ideen über eien beschreibende und zergliedernde Psychologie" (1894), in *Gesammelte Schriften,* vol. V (Berlin: Teubner, 1924). Köhler related his own notion of "insight" to Dilthey's "verständliche Zusammenhänge."

21. Gay, *Weimar Culture,* p. 59.

22. See, e.g., J. O. Urmson, *Philosophical Analysis: Its Development Between the Two World Wars* (Oxford: Oxford University Press, 1956).

23. Paul Forman, "Weimar Culture, Causality, and Quantum Theory, 191827: Adapation by German Physicists and Mathematicians to a Hostile Intellectual Environment," *Historical Studies in the Physical Sciences* 3: 1–115, 1971, and the references given there are of considerable interest in this connection. See also Gay, *Weimar Culture.*

24. Sixth meeting, *Transactions,* p. 28.

25. Silvan Schweber, "The Emergence of American Quantum Electrodynamics after World War II," *Osiris* 2: 265–302, 1986.

26. Alfred J. Marrow has written a biography, *The Practical Theorist: The Life and Work of Kurt Lewin* (New York: Basic Books, 1969).

27. Philosophical influences on Lewin are discussed by Miriam Lewin, "Kurt Lewin's View of Social Psychology: The Crisis of 1977 and the Crisis of 1927," *Personality and Social Psychology Bulletin* 3: 159–172, 1977; Erling Eng, "Looking Back on Kurt Lewin: From Field Theory to Action Research," *Journal of the History of the Behavioral Sciences* 14: 228–232, 1978.

28. K. Lewin, "Die Sozialisierung des Taylor Systems—Eine grundsätsliche Untersuchung zur Arbeits-u. Berufs-Psychologie," *Praktischer Sozialismus* 4: 5–34, 1919 (Karl Korsch, ed.).

29. Lewin, *Principles of Topological Psychology,* p. 3.

30. Lewin, "Cassirer's Philosophy of Science and Social Sciences," in Schilpp, ed., *The Philosophy of Ernst Cassirer,* pp. 269–288.

31. Kurt Lewin, *Vorsatz, Wille und Bedürfnis (mit Vorbemerkungen über die psychichen Kräfte und Energien and die Struktur der Seele)* (Berlin: Springer, 1926).

32. Gurwitsch, *Studies in Phenomenology and Psychology,* p. 56, from an article originally published in 1934 and making particular reference to Cassirer's *Das Erkenntnisproblem in der Philosophie und Wissenschaft der neueren Zeit* I (Berlin 1911), pp. 554–555.

33. Kurt Lewin, "The Conflict between Aristotelian and Galilean Modes of Thought in Contemporary Psychology," *Journal of Genetic Psychology* 5: 141–177, 1931.

34. Lewin, *Principles of Topological Psychology,* p. 18.

35. Ibid., pp. 43–44.

36. Kurt Lewin, "Time Perspective and Morale," in Lewin, *Resolving Social Conflicts,* pp. 103–124.

37. Ibid., p. 108.

38. See, however, Lewin's analysis of a marriage in K. Lewin, "Frontiers in Group Dynamics," *Human Relations* 1: 5–41, 1947.

39. Lewin, *Resolving Social Conflicts,* p. xvi.

40. Lewin, *Principles of Topological Psychology,* pp. vii–ix.

41. Ibid., p. viii.

42. Marrow, *The Practical Theorist,* pp. 74–75, indicates the part Köhler and Frank played in making this arrangement.

43. This appointment resulted from Frank's efforts and the backing he arranged with the Rockefeller General Education Board.

44. Marrow, *The Practical Theorist,* p. 403.

45. F. Terman to E. Boring, 11 April 1933, quoted by Sokal, "The Gestalt Psychologists in Behaviorist America."

46. Marrow, *The Practical Theorist,* p. 87.

47. Ibid., Appendix D, is an annotated bibliography of the work of Lewin and his group in Iowa.

48. K. Lewin, "The Research Center for Group Dynamics at M.I.T.," *Sociometry* 8: 126–136, 1945.

49. J. F. Brown, *Psychology and the Social Order* (New York: McGraw-Hill, 1936).

50. Lewin, *Resolving Social Conflicts,* p. 41.

51. Ibid., p. 47.

52. Ibid.

53. Kurt Lewin, "Frontiers in Group Dynamics: I. Concept, Method and Reality in Social Science; Social Equilibria and Social Change," and "II. Channels of Group Life; Social Planning and Action Research," *Human Relations* 1: 5–153, 1947.

54. Lewin did not insist on attempting to specify a complete social field. It was useful, in studying changes, to just measure one or a few variables as a function of time.

55. Lewin, "Frontiers in Group Dynamics," pp. 23, 147.

56. Jerome Frank, "Kurt Lewin in Retrospect—A Psychiatrist's View"; Eng, "Looking Back on Kurt Lewin"; Mary Henle, "Kurt Lewin as Metatheorist"; Steve Heims, "Kurt Lewin and Social Change"; Ralph K. White, "Has 'Field Theory' Been 'Tried and Found Wanting?,'" *Journal of the History of the Behavioral Sciences* 14: 223–246, 1978; Michael Sokal, "The Gestalt Psychologists in Behavioral America," *American Historical Review* 89: 1240–1263, 1984.

57. Lewin, "Frontiers in Group Dynamics: I," p. 40.

58. G. Lindzey and E. Aronson, eds., *Handbook of Social Psychology* (New York: Random House, 1985).

59. Alfred Schutz, *Collected Papers* (The Hague: Nijhoff, 1962–1966); see Gurwitsch's introduction to volume III.

60. See Coser, *The Function of Social Conflict,* p. 25, on Lewin's "strangely contradictory attitude toward group conflict."

61. Langdon Winner, one of the new generation of philosophers of technology, has offered two reasons for the neglect: First, "the astonishing hold of the idea of 'progress,'" and second, "the conventional view that the human relationship to technical things is too obvious to merit reflection." See Winner, *The Whale and the Reactor* (Chicago: University of Chicago Press, 1986), p. 5. For Wiener's work in the philosophy of technology, see Heims, *John von Neumann and Norbert Weiner*, chapter 13.

62. Mead to Erikson, 26 February 1947.

63. Letter from Resnik (on behalf of Erikson) to Heims, 24 August 1971; also Bateson to Heims, interview, 13 August 1968.

64. Letter from Festinger to Heims, 2 October 1973.

65. Alex Bavelas, "Mathematical Model for Group Structure," *Applied Anthropology* 7: 16–30, 1948.

66. Marrow, *The Practical Theorist,* pp. 143–146.

67. Eighth meeting, *Transactions,* pp. 1–44.

68. Ibid., p. 28.

69. Ibid., p. 42.

70. Bavelas to Heims, interview, Stanford University, 17 March 1968.

71. Bertram Schaffner, ed., *Group Processes,* second conference, Transactions, 9–12 October 1955 (New York: Josiah Macy Foundation, 1956), p. 25.

Chapter 10

1. Review of Wiener's *Cybernetics* (1948) by Köhler in *Social Research,* 125–130, 1951.

2. Jerome Bruner, *In Search of Mind: Essays in Autobiography* (New York: Harper & Row, 1983), pp. 87–88.

3. The definition and scope of "cognition" as a field of inquiry is given by Egon Brunswick, "Scope and Aspects of the Cognitive Problem," in *Contemporary Approaches to Cognition* (Cambridge: Harvard University Press, 1957), pp. 5–32.

4. Heinrich Klüver, "Psychology at the Beginning of World War II: Meditations on the Impending Dismemberment of Psychology Written in 1942," *Journal of Psychology* 28: 338–410, 1949.

5. Ibid., p. 405.

6. Klüver to Heims, letter, 7 October 1973.

7. Paul C. Bucy, "Heinrich Klüver," in *Psychopathology of Perception* (New York: Grune & Stratton, 1965), p. ix.

8. H. Klüver, "Stephen Polyak," University of Chicago *Medical Alumni Bulletin* 11: 3–4, Spring 1955.

9. H. Klüver, "Neurobiology of Normal and Abnormal Perception," *Psychopathology of Perception,* pp. 1–40.

10. H. Klüver, "Fragmentary Eidetic Imagery," *Psychological Review* 37: 441–458, 1930; "Eidetic Phenomena," *Psychological Bulletin* 29: 181–203, 1932.

11. H. Klüver, foreword to R. E. Schultes and A. Hofmann, *The Botany and Chemistry of Hallucinogens* (New York: Charles C Thomas, 1973), p. viii.

12. H. Klüver, *Mescal and Mechanisms of Hallucinations* (Chicago: University of Chicago Press, 1930, 1966), pp. 17, 47, 87 in the 1966 edition; the "etc., etc." is used by Heims to indicate Klüver's description of his vision goes on and on in a similar vein.

13. Ibid., p. 53.

14. Ibid., preface to 1966 edition.

15. Ibid., p. 9.

16. Andrew Weil, *The Natural Mind: A New Way of Looking at Drugs* (Boston: Houghton Mifflin, 1972), and *The Marriage of the Sun and the Moon* (Boston: Houghton Mifflin, 1980), although neither historical nor political, are interesting efforts at a broad perspective.

17. H. Klüver, *Behavior Mechanisms in Monkeys* (Chicago: University of Chicago Press, 1933, 1961).

18. Characteristics he considered were not restricted to physically measurable things, but also included response to sadness, attractiveness, aggressiveness—as he judged these qualities.

19. Klüver, *Behavior Mechanisms in Monkeys,* p. 5.

20. Klüver, *Behavior Mechanisms in Monkeys,* pp. 349–355; K. Koffka and M. Harrower, "Colour and Organization," *Psychologische Forschung* 15: 145, 193, 1931.

21. H. Klüver and P. C. Bucy, "An Analysis of Certain Effects of Bilateral Temporal Lobectomy in the Rhesus Monkey, with Special Reference to Psychic Blindness." *Journal of Psychology* 5: 33–54, 1938; H. Klüver, "Functional Differences between the Occipital and Temporal Lobes," in Lloyd Jeffress, ed., *Cerebral Mechanisms in Behavior* (New York: Wiley, 1951), pp. 147–199.

22. Klüver, addendum to "Psychology at the Beginning of World War II."

23. Stephan Chorover, *From Genesis to Genocide* (Cambridge: MIT Press, 1979), pp. 161–173.

24. Wiener, *Cybernetics,* pp. 157–158.

25. Ibid., p. 166.

26. Pitts and McCulloch, "How We Know Universals: The Perception of Auditory and Visual Forms," *Bulletin of Mathematical Biophysics* 9: 127–147, 1947; McCulloch, "Why the Mind Is in the Head," in *Cerebral Mechanisms in Behavior,* pp. 42–111. (Both articles are reprinted in *Embodiments of Mind.*)

27. Pitts and McCulloch, "How We Know Universals," in McCulloch, *Embodiments of Mind,* pp. 46–66; quote is on p. 65.

28. Jeffress, ed., *Cerebral Mechanisms in Behavior,* p. 32.

29. Ibid., p. 32.

30. Jerome Lettvin, foreword to *Embodiments of Mind,* 2nd ed., 1988.

31. C. K. Ogden and I. A. Richards, *The Meaning of Meaning* (London: Routledge and Kegan Paul, 1923.

32. Koffka and Harrower, "Colour and Organization."

33. Harrower to Heims, interview, Long Island, New York, 22 August 1971.

34. Molly Harrower, *Appraising Personality* (New York: Simon and Schuster, 1951, 1964); *The Practice of Clinical Psychology* (Springfield: Thomas, 1961).

35. Private communication from several regular conferees.

36. Bateson to McCulloch, 27 November 1946.

37. Sokal, "The Gestalt Psychologists in Behaviorist America."

38. Ibid., p. 1243.

39. Northrop to McCulloch, 25 February 1947.

40. S. Heims, "Encounter of Behavioral Sciences with Machine-Organism Analogies in the 1940s," *Journal of the History of the Behavioral Sciences* 11: 368–373, 1975.

41. Forman, "Weimar Culture, Causality and Quantum Theory," examines similar cultural biases influencing quantum theory.

42. W. Köhler, *The Task of Gestalt Psychology* (Princeton: Princeton University Press, 1966), p. 66.

43. John von Neumann, *The Computer and the Brain* (New Haven: Yale University Press, 1958), pp. 70–73; Arturo Rosenblueth, *Mind and Brain: A Philosophy of Science* (Cambridge: MIT Press, 1970), chapters 5 and 6.

44. McCulloch to Bateson, 7 December 1946; Lewin to McCulloch, 3 December 1946; McCulloch to Lewin, 9 December 1946.

45. Köhler to McCulloch, 2 June 1947.

46. McCulloch to Rosenblueth, 6 June 1947.

47. Köhler to McCulloch, 2 June 1947.

48. Köhler to McCulloch, 9 October 1947.

49. Köhler, *The Task of Gestalt Psychology*, p. 108.

50. Hans Lukas Teuber's father, Eugen Teuber, had been associated with Köhler for a time. He had been Köhler's predecessor on the Canary Islands and had set up the station for observation of anthropoid apes for the Prussian Academy of Sciences just before the First World War. His and Köhler's stays overlapped, and they did some joint experimental work there.

51. McCulloch to Teuber, 10 December 1947.

52. Teuber to Heims, interview, 30 October 1968; Harrower to Heims, interview, 22 August 1971; Teuber to McCulloch, 10 November 1947.

53. Teuber to Heims, interview, 30 October 1968.

54. Köhler, *The Task of Gestalt Psychology*, pp. 40–41.

55. Ibid., p. 41.

56. Köhler and H. Wallach, "Figural After-Effects," *American Philosophical Society Proceedings* 88: 269–357, 1944; Köhler and D. Emery, "After-Effects in the Third Dimension of Visual Space," *American Journal of Psychology* 60: 159–201, 1947.

57. The symposium was held at the California Institue of Technology during the week of 20–25 September 1948; the published proceedings are Jeffress, ed., *Cerebral Mechanisms in Behavior.*

58. Ibid., p. 231.

59. Ibid., p. 230.

60. Ibid., p. 70.

61. Ibid., p. 57.

62. Ibid., p. 24.

63. Seventh meeting, *Transactions*, p. 41.

64. Ibid., pp. 40–41.

65. Nilo Lindgren, "The Birth of Cybernetics," *Innovation* 6: 12–25, 1969.

66. W. Pitts and W. McCulloch, "The Statistical Organization of Nervous Activity," *Biometrics* 4: 91–99, June 1948.

67. *Social Research* 125–130, 1951 (my emphasis).

68. Lettvin, Maturana, McCulloch, and Pitts, "What the Frog's Eye Tells the Frog's Brain," *Proceedings of the I.R.E.* 47: 1940–1959, 1959. Reprinted in McCulloch, *Embodiments of Mind.*

69. Bruner, *In Search of Mind,* pp. 87–88.

70. Ibid., p. 97.

71. Ibid., p. 104.

72. Daniel Andler, "Cognitivism—Orthodox and Otherwise. A New Phase?," Proceedings of the European Philosophy Conference, Man in the Age of Technology, held in Athens, 24–28 June, 1985; Howard Gardner, *The Mind's New Science* (New York: Basic Books, 1985); David Rumelhart, James McClelland and the PDP Research Group, *Parallel Distributed Processing: Explorations in the Microstructure of Cognition,* 2 volumes (Cambridge: MIT Press, 1986); Bruner, *In Search of Mind;* Sigmund Koch and David Leary, eds., *A Century of Psychology as Science* (New York: McGraw-Hill, 1985), pp. 224–283, 392–453; Ulric Neisser, *Cognitive Psychology* (New York: Appleton-Century-Crofts, 1967), *Cognition and Reality* (San Francisco: Freeman, 1976), all trace the recent developments in the psychology and epistemology of the cognitive science approach to perception.

73. Rumelhart, McClelland, et al., *Parallel Distributing Processing.*

74. M. Waldrop, "Artificial Intelligence," *Science* 223: 802–805, 1983.

75. Newell, "Duncker on Thinking: An Inquiry into Progress in Cognition," pp. 392–419, Koch and Leary, eds., *A Century of Psychology as Science,* p. 396.

76. Gardner, *The Mind's New Science,* p. 6.

77. Quoted in McCulloch, *Embodiments,* 2nd ed., p. v.

78. For the Gibsonian approach the interested reader is referred to Edward Reed, *James J. Gibson and the Psychology of Perception* (New Haven: Yale University Press, 1988); James J. Gibson, *The Ecological Approach to Visual Perception* (Boston: Houghton Mifflin, 1979); *The Senses Considered as Perceptual Systems* (Boston: Houghton Mifflin, 1966); *The Perception of the Visual World* (Boston: Houghton Mifflin, 1950).

79. J. J. Gibson, "Conclusions from a Century of Research on Sense Perception," in Koch and Leary, eds., *A Century of Psychology as Science,* p. 229.

80. Quoted by E. H. Gombrich, *The New York Review of Books,* 19 January 1989, p. 13.

81. Gibson, *The Senses Considered as Perceptual Systems,* p. 3.

82. Ibid., pp. 5, 33.

Chapter 11

1. Wiener presents his synthesis especially in *Cybernetics* and *The Human Use of Human Beings.*

2. Bateson, "Circular Causal Systems in Society," in the Margaret Mead archives (box 104), Library of Congress. The paper was presented at the New York Academy of Sciences Conference on Teleological Mechanisms, 21–22 October 1946, and appeared as a preprint, but was not included in the published transactions of the meetings.

3. Bateson on various occasions referred to the mathematicians and engineers responsible for the concepts and technologies of cybernetics collectively as physicists.

4. "Circular causal systems in society."

5. Bateson to Heims, 7 June 1968. His reference is to James D. Watson's book, which had just appeared.

6. Bateson to McCulloch, 25 October 1962.

7. Ibid.

8. Long to Bateson, 14 February 1963.

9. Bateson, *Steps,* p. 487.

10. Ibid., p. xv.

11. Ibid., p. 153.

12. Thanks to H. Millard Clements for the appropriate notion of shaman. See also *The Esalen Catalog,* January–June 1981, as well as various issues of the *CoEvolution Quarterly* for 1975, 1976 and 1977 describing Bateson in his later years.

13. Gregory Bateson, *Mind and Nature: A Necessary Unity* (New York: Bantam, 1979), p. 3.

14. Ibid.

15. Ibid., p. 163.

16. Bateson, *Our Own Metaphor,* p. xiii.

17. Ibid., p. 311.

18. Arthur L. Blumenthal, *The Process of Cognition* (New York: Prentice-Hall, 1977). John Stroud, "The Fine Structure of Psychological Time," in H. Quastler, ed., *Information Theory and Psychology* (Glencoe: Free Press, 1955), pp. 174–207.

19. Stroud to Heims, 19 October 1972.

20. Sixth meeting, *Transactions;* also *Hixon Symposium,* pp. 97–98.

21. "Introduction" by Warren McCulloch in Kenneth Craik, *The Nature of Psychology* (Cambridge: Cambridge University Press, 1966).

22. Stroud, "The Fine Structure of Psychological Time."

23. Ibid., pp. 174, 180.

24. Punctuation and line breaks supplied by me.

25. James Lovelock, *Gaia: A New Look at Life on Earth* (Oxford: Oxford University Press, 1979); Lynn Margulis and Dorion Sagan, *Microcosmos* (New York: Simon and Schuster, 1986).

26. F. S. C. Northrop, *The Meeting of East and West* (New York: Macmillan, New York, 1946), p. 164.

27. Northrop to Heims, interview, 5 November 1968, New Haven.

28. P. A. Schilpp, ed., *Albert Einstein: Philosopher-Scientist* (New York Library of Living Philosophers, 1951), p. 683.

29. H. Margenau, "Causality and Modern Physics," *The Monist* 41: 1–36, 1931; Northrop, "The Concept of Probability in Quantum Mechanics," *Philosophy of Science,* 3: 215–232, 1936.

30. Forman, "Weimar Culture, Causality, and Quantum Theory."

31. F. S. C. Northrop, *The Logic of the Sciences and the Humanities* (New York: Macmillan, 1948), p. 119.

32. Ibid., p. 165. The article containing the quotation was originally presented at a symposium on 25 June 1940.

33. Northrop, *Logic,* chapter X; comments originally made in December 1938 at a symposium.

34. F. S. C. Northrop, "Philosophy and World Peace," in L. Bryson et al., eds., *Approaches to World Peace. Conference on Science, Philosophy and Religion: 4th Symposium* (New York, 1944), pp. 642–652.

35. Ibid., p. 646.

36. Ibid., p. 651.

37. F. S. C. Northrop, "Toward a Religion with Worldwide Transforming Power," in L. Bryson et al., eds., *Conflicts of Power in Modern Culture, Conference on Science, Philosophy and Religion, 7th Symposium* (New York: 1946), pp. 357–365.

38. A. Einstein, "Why Socialism?" *Monthly Review,* May 1949, reprinted in L. Huberman and P. M. Sweezy, *Introduction to Socialism* (New York: Modern Reader Paperback, 1968).

39. The paper is reprinted as chapter XXI of *Logic;* it was first published in *Social Science,* July 1947.

40. F. S. C. Northrop, "The Neurological and Behavioristic Psychological Basis of the Ordering of Society by Means of Ideas," *Science* 107: 411–416, 1948.

41. Ibid.; F. S. C. Northrop, *Philosophical Anthropology and Practical Politics* (New York: Macmillan, 1960), especially chapters 2, 3, and 4.

42. F. S. C. Northrop, ed., *Ideological Differences and World Order* (New Haven: Yale University Press, 1949), p. 424.

43. Northrop, *Man, Nature and God,* p. 48.

44. McCulloch, letter "To the Members of the Feedback Conference on Teleological Mechanisms," 2 February 1952.

45. Northrop, *Philosophical Anthropology.*

46. McCulloch interpreted Wiener's unwillingness in a letter to Northrop on 15 April 1947, part of which is reproduced in Heims, *John von Neumann and Norbert Wiener,* p. 328.

47. Mead, *Coming of Age in Samoa* 2nd ed., p. 145.

48. Mead, "The Comparative Study of Cultures and the Purposive Cultivation of Democratic Values," in L. Bryson et al, eds., *Science, Philosophy and Religion in their Relation to the Democratic Way of Life, Science, Philosophy and Religion: Second Symposium* (New York, 1942), pp. 57–58.

49. Mead, "The Comparative Study of Cultures and the purposive cultivation of Democratic Values, 1941–1949," in L. Bryson et al., eds. *Perspectives on a Troubled Decade 1939–1949, Science, Philosophy and Religion: Tenth Symposium* (New York, 1949), p. 91.

50. Ruth Benedict, review of *The Meeting of East and West* in *The New Republic,* 9 September 1946, pp. 299–300.

51. Part 4 of "The Kingfishers," dated 20 July 1949, in Charles Olson, *Archaeologist of Morning* (New York: Grossman, 1973), p. 374.

Chapter 12

1. Richard J. Bernstein, *Beyond Objectivism and Relativism: Science, Hermeneutics, and Praxis* (Philadelphia: University of Pennsylvania Press, 1983), p. 223.

2. Bateson, *Steps,* p. 43.

3. Wiener, *Cybernetics,* p. 160.

4. Warren McCulloch, "Summary of the Points of Agreement Reached in the Previous Nine Conferences on Cybernetics," tenth meeting, *Transactions,* pp. 69–80.

5. See Heims, *John von Neumann and Norbert Wiener,* for the reaction of von Neumann and Wiener, to military applications.

6. See also Edward, "Technologies of the Mind: Computers, Power, Psychology, and World War II."

7. G. E. Hutchinson, "Circular Causal Systems in Ecology," *Annals of the New York Academy of Sciences* 50: 221–246, 1948; Peter Taylor, "Technocratic Optimism, H. T. Odum, and the Partial Transformation of the Ecological Metaphor after World War II," *Journal of the History of Biology* 21: 213–244, 1988.

8. Lovelock, *Gaia;* Margulis and Sagan, *Microcosmos.*

9. Macy conferees understood by 1953 that, as von Neumann had pointed out, a proper automaton model of the brain would rely on parallel processing. McCulloch refers to this in tenth meeting, *Transactions,* pp. 78–79.

10. David Rumelhart, James McClelland and the PDP Research Group, *Parallel Distributing Processing: Explorations in the Microstructure of Cognition,* 2 volumes (Cambridge: MIT Press, 1986).

11. Daniel J. Amit, *Modelling Brain Function* (Cambridge: Cambridge University Press, 1989).

12. Stewart Brand, *The Media Lab: Inventing the Future at MIT* (New York: Viking, 1987), p. 11. Brand spent six months at the Media Lab to learn about its activities and prepare his book, which I have relied on in my discussion; I also spoke to some of its faculty and students.

13. Ibid., p. 123.

14. Joseph Weizenbaum, *Computer Power and Human Reason: From Judgment to Calculation* (San Francisco: Freeman, 1976); Sherry Turkle, *The Second Self: Computers and the Human Spirit* (New York: Simon and Schuster, 1984).

15. Langdon Winner, *Autonomous Technology: Technics-out-of-Control as a Theme in Political Thought* (Cambridge: MIT Press, 1977).

16. Turkle, *The Second Self,* pp. 246–247.

17. Morris Berman, *The Reenchantment of the World* (Ithaca: Cornell University Press, 1981).

18. Sharon Lamb, "Acts without agents. Harm without guilt" (submitted for publication)

19. Millard Clements, "Technology and Education" in *Great Ideas in American Schooling.* Paul Robinson, editor (West Lafayette, Indiana: Kappa Delta Pi, 1989), pp. 96–116, and Clements, "Note from the Editor" in Theory and Research in Social Education, 18, 91–99, 1990.

20. Shoshana Zuboff, *In the Age of the Smart Machine* (New York: Basic Books, 1988); David Noble, *The Forces of Production* (New York: Knopf, 1984).

21. P. Dumouchel and J. Dupuy, eds., *L'auto-organisation de la physique au politique* (Paris: Seuil, 1983); Jean-Pierre Dupuy, *Ordres et Désordres* (Paris: Seuil, 1982); Henri Atlan, *Entre le cristal et la fumée* (Paris: Seuil, 1979); I. Prigogine and I. Stengers, *La Nouvelle Alliance* (Paris: Gallimard, 1979); H. von Förster and H. Zopf, eds., *Principles of Self-Organization* (New York: Pergamon, 1962); Humberto Maturana and Francisco Varela, *Autopoiesis and Cognition* (Boston: Reidel, 1980); M. Zeleny, ed., *Autopoiesis. A Theory of Living Organization* (New York: Elsevier, 1981); S. Beer, *The Brain of the Firm* (London: Allen Lane, 1972); G. Pask, *Conversation Theory* (New York: Elsevier, 1976); Peter Cariani, "On the Design of Devices with Emergent Semantic Functions," Ph.D. dissertation (SUNY at Binghamton, 1989).

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